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PREFATORY NOTE

The papers herewith presented are reprinted from the pages of the *Psychological Review*.

Reprinted from THE PSYCHOLOGICAL REVIEW, Vol. VIII., No. 3, May, 1901.]

FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

THE MONAURAL LOCALIZATION OF SOUND.

BY PROFESSOR JAMES ROWLAND ANGELL AND DR. WARNER FITE.

The present paper reports a series of observations upon the capacities of auditory localization in a person entirely deaf in one ear. In several of the experiments we have made parallel observations upon a person of normal hearing, and the cases are presented comparatively, in order to bring out as clearly as possible the peculiarities of monaural localization.

I.

We may conveniently preface the report of our experiments with a brief survey of the current theories of binaural localization.

Two general types of theory have been advanced to account for the ability to localize sounds. One of these, with which the names of Preyer and Münsterberg are associated, refers auditory localization to the action of the semicircular canals.[1] This view has as yet met with no general acceptance. The other type of theory, which is widely maintained by psychologists and physiologists, is formulated with admirable precision in the following words of the British scientist, S. P. Thompson:[2]

[1] Preyer, *Pflüger's Archiv*, Bd. 40, S. 586. Münsterberg, Beiträge z. Psychologie, Heft II., S. 182. These writers differ distinctly in their conceptions of the precise *modus operandi* of the canals.

[2] S. P. Thompson, *Philosophical Magazine*, January-June, 1882, p. 415. This article contains an excellent critical discussion of the theories of Luca, Steinhauser, Graham Bell, Mayer, Mach, Lord Rayleigh and others. Among the best of recent investigations are those of Pierce, PSYCHOLOGICAL REVIEW, Vol. I., p. 461, and Matsumoto, *Yale Studies*, Vol. V., p. 1.

"Judgments as to the direction of sounds are based, in general, upon the sensations of different intensity in the two ears; but the perceived difference of intensity upon which a judgment is based, is not usually the difference in intensity of the lowest or fundamental tone of the compound sound (or 'clang'), but * * * the difference in intensity of the individual tone or tones of the clang for which the intensity-difference has the greatest effective result in the quality of the sound."

This statement obviously makes differences in the intensity and quality of the sound sensations perceived by the two ears the fundamental factors in our localizations. Many of the adherents of this general view emphasize the importance of the intensity element, as if that were really the basal fact, and mention difference in quality as though it were somewhat incidental and distinctly of secondary importance. Thompson's formulation puts the matter in an undoubtedly more accurate form. The significant objective difference in sound waves reaching the two ears with unequal intensity is not so much in the amplitude of the fundamental vibration, which gives the sound its pitch, as in the amplitude of the upper partials, which contribute very largely to the timbre or quality of an auditory complex.[1] Generally speaking, then, a difference in the intensity of the sound stimulations reaching the two ears results in the consciousness of a difference in the auditory quality of the two sensations, rather than in a mere perception of inequality in the intensity of the two stimulations. Of course this cannot be the case with pure tones. But it is to be remembered that pure tones are notoriously difficult to localize with any approach to accuracy. Our auditory orientation corresponds, therefore, to the intensive and qualitative difference in the sound sensations of the two ears, and of these the qualitative differences are seemingly much the more conspicuous in consciousness.

[1]Thompson (*loc. cit.*) inclines to accept Lord Rayleigh's view that this modification of the partials is due chiefly to the effects upon the sound waves of the bones of the skull, as against Mach's contention that the pinnæ are mainly responsible for the phenomena. Our observations, as will be seen, tend on the whole to confirm Lord Rayleigh's theory on this point. The relative importance of the two factors varies with different objective positions of sounds, and both of them are at times clearly operative.

Unless modified by explanatory corollaries, a formulation phrased in this manner is unquestionably open to the criticism urged by Stumpf[1] and others who follow him. It is somewhat more precise than the earlier statements of v. Kries[2] and the similar formula of Matsumoto,[3] quoted approvingly by Ladd.[4] But at best it leaves much to be desired on the score of accuracy of expression. When dealing with intensity differences, the explanation which the statement advances requires a perception of the intensity of the sound in one ear as greater or less than the intensity of the sound in the other ear. Or, when expressed in terms of quality, it requires a perception of the quality of the sound in one ear different from the quality of the sound heard by the other ear. Neither statement corresponds exactly to the facts. We are conscious of only one sensation, only one intensity and only one quality. This single sensation, with its intensity-quality attributes, is referred to some objective point in space. We may at times connect it with the predominant or exclusive action of one ear. But in no case is there any real comparison or analysis of the two intensities or qualities proceeding from the two ears.

Undoubtedly on some occasions the physical and physiological conditions resulting in the stimulation of the two ears with unequal intensities give rise to psychical processes involving localization of the sound. And undoubtedly, also, the physical and physiological conditions which are most often encountered are such as accompany differences in the quality of the sounds falling upon the two ears; with which, in turn, localizations may be connected. But this is a very different proposition from the one above quoted, with its implication of a conscious comparison of the intensities and qualities of sounds heard by the two ears.[5]

[1] Stumpf, Tonpsychologie, Vol. II., p. 50, ff.

[2] v. Kries, *Arch. f. Anat. und Physiol.* (Physiol. Abth.), 1877, p. 329. This author has, in a later publication (*Zeitsch. f. Psychol. und Physiol.*, Vol. I., p. 235) criticising Preyer and Münsterberg, made a much broader statement, including more explicit reference to the effects of quality upon auditory localization.

[3] *Loc. cit.*

[4] Ladd, Outlines of Descriptive Psychology, p. 191.

[5] We omit discussion of sounds heard as double, either in binaural or monaural hearing. They do not involve the point we are here discussing. Cf. Stumpf, *loc. cit.*

We learn to localize sounds on the basis of differences among them, which are partly differences of intensity and partly differences of quality, involving sometimes variations in apparent pitch. These differences in intensity and quality have their physical and physiological basis largely in the differences between the stimulations of the two ears. But these physical and physiological differences are not reported in consciousness in the form of sensations connected with the separate ears. The theories which refer auditory localization immediately to these factors are, therefore, primarily engaged with the physical and physiological pre-conditions of the process.[1] Intensity and quality are unquestionably involved in conscious processes of auditory localization, to say nothing of motor and visual elements.[2] But these formulations with which we have been dealing are in point of fact only secondarily concerned with the strictly psychical phases of the phenomena. We shall take cognizance, in connection with our own work, of both kinds of facts.[3]

II.

We are now ready to begin the account of our experiments, and we shall anticipate at the outset our most important conclusion, in the belief that we may thus materially clarify our subsequent descriptions and discussions.

In the case of monaural hearing there can obviously be no question of the operation of the intensity factor in the form considered in the previous section. Provided the position of the head is fixed, changes in apparent intensity are, if unaccompanied by changes in quality, wholly ambiguous in monaural

[1] This procedure is much after the manner of the current color theories.

[2] Münsterberg's account (*loc. cit.*) of the part played in auditory localization by reflex and semi-reflex movements corresponds closely to the accepted view of the more careful psychological writers. This statement in no wise implies assent in such quarters to his theory of the dependence of these movements upon the action of the semicircular canals.

[3] It is no part of the present undertaking to go into the merits of the nativistic-empiristic space controversy, and our investigations touch only indirectly, if at all, the problem of the intrinsic voluminousness and extensity of auditory sensations. Our immediate interest is in the fundamental physical and psychical conditions upon which monaural localization depends, and such localization is clearly in a space of the conventional visual-tactual-kinæsthetic kind and not in one of a purely auditory character.

hearing. They may mean change of distance, change of direction, change of actual objective intensity with or without change of either distance or direction, or finally any combination of these factors, and the subject has absolutely no reliable means of determining which of these alternatives is really involved in any specific case.

The qualitative differences already described in the quotation from Thompson are differences connected with variations in the stimulations of two ears. We find that qualitative differences in sounds coming from different directions and falling upon one ear are quite adequate to serve for localizations of considerable accuracy. Monaural hearing involves apparently a system of local signs based on qualitative differences, which are rapidly acquired for any given series of sounds, when not possessed at the time the sounds are first heard. This development is quite independent of any knowledge imparted through channels other than the auditory sensation itself, and independent of any assistance from the turning of the head, which might confirm, by changing the perceived intensity, the original judgment based on quality.

The subject of the experiments is a man of thirty, who lost the use of his left ear, when four years of age, through ulceration of the middle ear consequent upon an attack of scarlet fever. The tympanic membrane was practically destroyed and a scar has replaced it. How far the bones of the middle ear are affected it is not possible to state. The malleus appears, however, to be in place. The internal ear was probably attacked by the disease, of which all traces save deafness disappeared at the age of fifteen. In any case the deafness on the affected side seems to be complete. To assure ourselves of this as far as possible, the subject was examined by an experienced aurist. The Weber test applied with the C fork resulted in strong lateralizing in the intact ear. For both high and low tones complete deafness was demonstrated. The C_4 fork was heard when the intact ear was tightly closed. But it was heard with equal distinctness when the defective ear was also closed, showing that this ear had not participated in the transmission. The test for the duration of bone conduction could not be made,

because of the entire normality of one ear. The evidence points almost conclusively to lesion in the labyrinth.

The subject reports that, since the onset of the disease, he has never had the slightest reason to suppose he hears at all with this ear. The whole region of the external ear is, as compared with the intact side, rather numb in feeling. The normal ear is somewhat unusually acute. The intensive threshold is lower than that of the average person, and the sensibility for tone runs from 16 vibrations per second up to 35,000 per second. The musical capacity is good, with a limen for pitch discrimination rather better than the average. In no respect, however, is the hearing very remarkable.

The subject employed for comparison is entirely normal in every particular and requires, therefore, no further description.

Apparatus.—The apparatus used in these experiments was substantially the same as that used by Pierce and Matsumoto.[1] Our purpose was to give the subject sounds of the same quality and intensity from different directions. Equal intensity was secured by use of the same source of sound—in our experiments a telephone—placed always at the same distance from the head. To secure a uniform distance Pierce and Matsumoto used a spherical cage supported by standards, and so placed that the head of the subject, which was supported by a head-rest clamped to the back of his chair, should be in the center of the sphere. We found it more convenient to suspend our cage (diameter 4 ft.) from the gas-fixture by means of a wire. Using the wire as an axis of rotation, the experimenter could then stand always at the same place and, after attaching the telephone at the proper height, noiselessly turn the cage the required distance. Thus no suggestion of the location of the sound was given by the movements of the experimenter. In order to secure a constant position for the axis of rotation two uprights were fastened to the floor, and the equatorial circle of the sphere rested against them when the telephone-click was given. It should be added that we were thus able to reduce the framework of the sphere to two circles, the equatorial circle

[1] *Op. cit.*

and one vertical circle, the latter interrupted below to make room for the body of the subject.

In previous experiments upon auditory localization the subject was seated with the center of his head (the intersection of the visual axis with the line joining the two ears and named the auditory axis) in the center of the sphere. But since our subject was monaural it was at first thought necessary, in order that all stimuli might reach him with the same intensity, to center his intact ear (the right ear) rather than the head. This position was afterwards abandoned, for reasons to be mentioned later. It was also found convenient to nail a strip of wood to the floor, parallel with the front legs of the chair, and another strip in the same direction across the under side of the chair with the ends projecting. By placing his feet upon the first strip and his hands upon the projecting ends of the second, the subject could keep himself informed of the position of his body, in reference to which his localization was made. Without these aids his sense of bodily position seemed sometimes uncertain and seemed also to have a disturbing effect upon his localization of sounds.

We began by giving the sound twice for each test, but after the first series it was repeated often six to ten times, until the subject was satisfied with his judgment. The effect of this was simply to secure a normal condition of attention. The subject was then required to name the point from which the sound came, knowing of course the number and location of the points from which it might be given. The sound was given by the experimenter, who closed a circuit by means of a noiseless mercury-contact key. In some of the later experiments, however, it was found convenient to place a contact-key in the hands of the subject, allowing him to give the sound after being informed that the telephone was in position. This method would be recommended for future experiments as securing the best conditions of preparation. To avoid any suggestion of regularity in the order of the points given, the order for each day was determined in advance by shuffling cards bearing the names of the points.

Except where otherwise stated, the electric current used was from three Gonda cells. The current was passed through the primary circuit of an induction coil, in whose secondary circuit

was placed a d'Arsonval galvanometer. The galvanometer enabled us to verify the constancy of the current and the consequent constancy in the intensity of the telephone sound.

The method of notation used by Matsumoto appeared to be the most convenient. In his system the points are designated by significant letters. But we have not included in our system all the points used by him; and since the number of points from which the sound could come has an important relation to the numerical value of the error, we shall not be able to give satisfactory comparisons between his results and our own.

Our system of points was contained in three horizontal circles: an equatorial circle, whose plane passes through the center of the head, a circle 45° above the equatorial circle, and a third 45° below. There were in all sixteen points.

The points on the equatorial circle are designated as follows: *f* (front), the intersection of the visual axis of the subject with the circle in front of the subject; *b* (back), 180° from *f*; *r* (right), the intersection of the auditory axis with the circle on the right; *l* (left), 180° from *r*; *fr*, 45° from *f* and *r*; *br*, 45° from *b* and *r*; *fl*, 45° from *f* and *l*; *bl*, 45° from *b* and *l*.

The points on the upper circle are *fo*, *bo*, *ro*, and *lo*, *o* (over) denoting a point on the upper circle; those on the lower circle are *fu*, *bu*, *ru*, and *lu*, *u* (under) denoting a point on the lower circle.

Fifteen series of experiments were performed.[1] *Series I.* was for the purpose of determining the general characteristics of monaural as compared with binaural localization. A set of 480 experiments was made upon the monaural subject, *A*, and for purposes of comparison under approximately identical conditions a set of 232 was made upon *B*, a binaural subject. The order of each day's experiments was 24 upon *A*, then 24 upon *B*, followed by 24 more upon *A*. The results obtained from *A* are given in Table I., those from *B* in Table II.

The following points of comparison should be noted:

1. In the binaural localizations there were only two instances in which sounds in the median plane and the right and left hemispheres were confused with each other. Matsu-

[1] The series are numbered in the order in which they were performed.

moto's subject made no errors of this kind. But 72 such errors are recorded for *A*, all of which, however, are confusions between the median plane and one of the two hemispheres; in no case are the hemispheres confused with each other.

2. The number of *A's* correct judgments is 227 out of 480

TABLE I.

Position of sound.	Direction in which the sound is perceived to lie. f	fo	bo	b	bu	fu	fl	lo	bl	lu	l	fr	ro	br	ru	r	No. given.
f	17	6		2	1	1								3			30
fo		3	5	4								1	3	10		4	30
bo			20										2	5		3	30
b			1	25										4			30
bu				1	28				1								30
fu				18	2	1	1							7		1	30
fl			1	14	2		5	1	7								30
lo			3	2				9	15		1						30
bl								6	1	3	20						30
lu								1	12	2	15						30
l							1	2	2		25						30
fr												10	13			7	30
ro			2		1								14	6	1	6	30
br				1									1	22		6	30
ru					1									4	23	2	30
r									1					8		22	30
															Total		480

NOTE.—Heavy faced figures indicate right cases. Figures outside the larger squares indicate confusions of middle and right and left.

judgments, or 47.3%. *B's* correct judgments were 168 out of 232 judgments, or 72.4%.

It will be of some interest to compare with these figures the results obtained by Matsumoto, though, as we have already noted, the difference in the number of points from which choices may be made renders the two results not exactly comparable; for the

points common to his system and ours he obtained out of 800 trials 483 right cases, or 60.4%.

3. The distribution of right judgments for *A* is: median plane 94, left hemisphere 42, right hemisphere 91. Noting that the median plane contains 6 points as against 5 in each hemi-

TABLE II.

Position of sound.	Direction in which the sound is perceived to lie. f	fo	bo	b	bu	fu	fl	lo	bl	lu	l	fr	ro	br	ru	r	No. given.
f	12	10															22
fo	1	15															16
bo		7	6														13
b				16	1												17
bu				7	15												22
fu		1	7	5	9	3											25
fl							12	4									16
lo								7			1						8
bl								3	10		1						14
lu									1	9							10
l								1			15						16
fr												12	1			1	14
ro													10				10
br				1										11			12
ru					1										7		8
r														1		8	9
															Total		232

NOTE.—Heavy faced type shows right cases. Figures outside the larger squares indicate transpositions of middle and right and left.

sphere, the ratio of right judgments is as follows: middle 15.7, left 8.4, right 18.2; that is, about half as many right cases occur in the left hemisphere as in either the middle or right. It is to be remembered that the intact ear was on the right. The distribution of right cases for *B* was as follows: 58% of those

in the middle plane, 91 % of those on the left, and 83% of those on the right.

4. The points most often confused with each other were, for *A*, as follows: *l* and *bl* 22 (times), *lo* and *bl* 21, *b* and *fu* 18, *bl* and *lu* 15, *lu* and *l* 15, *fl* and *b* 14, *fr* and *ro* 13, *fo* and *br* 10. For *B* such points were: *f* and *fo* 11, *fu* and *bu* 9, *bu* and *b* 8, *fo* and *bo* 7. It should be noted that *B's* chief confusions are between points in the median plane, while those of *A* are distributed generally. Since the number of experiments upon the two subjects was unequal, a numerical comparison is not intended; the figures are given merely to show the most important confusions of each subject in order of prominence.

5. These confusions may be further classified as follows: Transpositions forward, *A* 29 (times), *B* 9; transpositions backward, *A* 84, *B* 26. The tendency of both was to locate sounds backward rather than forward of their true position.

6. The following shows the number of errors, classified according to amount of error (disregarding direction). It should be noted that, in our method, the least ascertainable error was 45°. This undoubtedly over-emphasizes the actual error; but the only means of overcoming the difficulty promised more serious complications and was therefore disregarded.

	45°	90°	120°	135°	180°
A	164	26	21	40	2
B	35	17		5	7

The following shows the same, classified according to percentage of the whole amount of error:

	45°	90°	120°	135°	180°
A	64.8	10.3	8.3	15.8	.8
B	54.7	26.6		7.8	10.9

Series II. was performed in order to show the effect of practice. It seemed clear (the subject being in ignorance of the results obtained from himself) that *A* had improved rapidly in accuracy during the progress of Series I. A second series, of 271 experiments, was then given to ascertain the extent of this improvement. For convenience, five points (*bu*, *l*, *br*, *ru*, and *r*) which had already shown a large percentage of correct

cases were omitted in Series II. These points gave in Series I. 120 right and 30 wrong cases. Subtracting these amounts from the totals we have, for the remaining 11 points, 107 right and 223 wrong. The results for these points in Series II. were 139 right and 132 wrong, the number of experiments being about evenly distributed among the different points. A comparison of the two series shows, then, for *A*: in the first series 32.4% right, 67.6% wrong; in Series II., 51.3% right, 48.7% wrong. The distribution and general character of the errors was about the same as in Series I. No further experiments were made upon *B*.

Series III. and IV.—Having ascertained that *A's* capacity for localization was in general not greatly inferior to that of binaural subjects, it became our object to determine, if possible, just what his data for localization were. It was suggested that he might be guided by cutaneous sensations from his head and neck. Accordingly, two series of experiments were performed in the endeavor to eliminate this factor. The head and neck were covered with a camera-cloth, in which a slit had been made to allow the right ear to protrude. In Series III. the camera-cloth was bound loosely to the head by a strap passed across the forehead and around the head. The results of this series appeared at first to give distinctly positive information. The number of right cases was only 109 out of 318 experiments, and not only were the hemispheres confused with the median plane, but left was a number of times transposed to right. The possibility remained, however, that the transpositions and other errors were due to the cutting off of partial tones, on which basis, perhaps, the localization was made, by the loose folds of the camera-cloth. Accordingly, another series (IV.) was tried, this time with the camera-cloth bound close to the head and face by means of straps passed around the head in three directions. The ratio of right cases (158 out of 310, or 51%) and the nature of the errors were neither of them materially different from those in series II. It seems probable, then, that cutaneous sensations constituted a small part, if any, in the data for localization, though it is necessarily uncertain how far the possible effects of the sound waves were checked by the camera-cloth.

It seems fairly certain, however, that the strong cutaneous stimulation afforded by the contact of the cloth with the skin must have overpowered any effects due to the sound waves alone.

In this connection we should mention a later series (IX.), which was performed with the same end in view. Both ears were fully closed with oily putty as far as the outer rim of the pinnæ. The battery power was increased from three to five cells. 57 positions were then given, about evenly distributed among *f*, *b*, *r*, *l*. The only noticeable difference from the normal was the almost universal transposition of *f* to *b*.[1] Otherwise the localizations did not seem to be seriously affected. The net result of these three series was thus merely negative. It is possible, however, that interesting results in this direction might still be obtained from a subject absolutely deaf in both ears.

Series V.—A reference to Series I. and II. shows that sounds in the median plane were transposed frequently to the right but practically never to the left. The subject, *A*, who remained in ignorance of the results, also remarked that sounds which were (correctly) localized as *f* and *fl* appeared to come from a point somewhat to the right of the point which, from his knowledge of the system of points, he judged to be their true position. These facts seemed to indicate that he localized a sound with reference to his head rather than his ear. Accordingly, a series was tried in which the center of the head was located at the center of the sphere. The results are given in Table III.

Compared with Series I. and II., the following facts should be noted:

1. There were only 12 cases of confusion between the median plane and the hemispheres, as compared with 72 in Series I. and 33 in Series II. Comparing the number of experiments with Series I., there should be 48 such cases; with Series II., there should be 39. We have to do here undoubtedly in some measure with continued practice effects. It is, however, extremely improbable that practice is wholly accountable for the result.

[1] Münsterberg (*op. cit.*) using a partial filling of wax for the external meatus and conch, decided that the pinnæ were chiefly important in the localization of sounds from in front. Compare also note above on page 226.

It seems more reasonable to suppose that the conditions of this series were intrinsically more favorable for monaural localization.

2. The number of correct judgments is 240 out of 322, or 75%, as compared with 47.3% in Series I., 51.3% in Series II., 60.4%

TABLE III.

Position of sound.	Direction in which the sound is perceived to lie. f	fo	bo	b	bu	fu	fl	lo	bl	lu	l	fr	ro	br	ru	r	No. given.
f	20																20
fo		16		7										1			24
bo			18													2	20
b				19										1			20
bu					19	1											20
fu	2			4		10								3			19
fl						1	16		3								20
lo		1						19									20
bl						1	17		0		2						20
lu									4	11	5						20
l							8	2	2		7						19
fr												19	1				20
ro													17		3		20
br	2													9		9	20
ru															20		20
r																20	20
														Total			322

NOTE.—Heavy faced type indicates right cases. Figures outside the larger squares indicate transpositions of the middle and right and left.

for Matsumoto's subject, and 72.4% for *B*. Taken as it stands the percentage is thus larger than any other recorded.

3. The distribution of right cases is: median plane, 102; right hemisphere, 85; left hemisphere, 53. Noting again that the number of points in the middle was 6, as compared to 5 in each hemisphere, the ratio is: median 17, right 17, left 11.

The greater proportion of error is naturally, as at the beginning, on the side of the deaf ear.

4. The main confusions are between *fl* and *bl*, 20; *fl* and *l*, 8; *br* and *r*, 9; *fo* and *b*, 7.

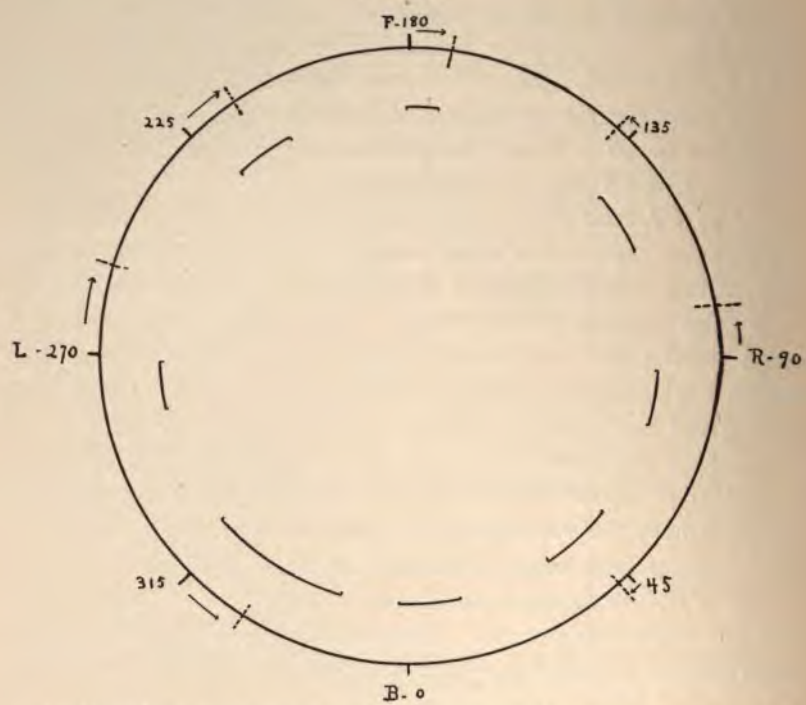
5. The most uncertain of all positions is *bl*, for which there are no right judgments. For some reason *l*, which was easily localized in Series I. and II., now partakes of the general uncertainty of all localizations in the left hemisphere. In this connection it should be noticed that *lu*, which has hitherto been very uncertain, now shows a fair percentage of right cases, the reason for which seemed to be that, in the first position of the body, *lu* was damped by the proximity of the subject's sleeve. This fact seems to have had a marked effect upon the apparent position of the sound. *fu*, which was close to the knees, was not at any time clearly identified, and was the only point seriously uncertain for *B*. These considerations point to the quality of the sound as a factor in localization.

Series VI. and VII. were merely confirmatory. Series VI. was for the purpose of eliminating as possible factors in localization the modifications of the sound due to the walls of the room and objects in the room. The subject had previously faced north; he now faced successively south, west and east, and in each position was given forty sounds, distributed equally among the four sound-positions most accurately localized (*f*, *r*, *b*, *bu*) and the four least accurately localized (*fu*, *l*, *bl*, *br*). A similar procedure was followed in Series VII., with the exception that here the variable factor was the intensity of the sound. The current had been furnished hitherto by three cells. In Series VII. the strength of the current was varied—two, three and five cells being used. Forty sounds were given with each arrangement of the cells—not, however, in separate groups, but in irregular order in one group, the subject not being informed of the strength of the current. No departure from the normal occurred in either series. This would again seem to indicate that the quality of the sounds, rather than the intensity of the fundamental tone in the sounds, was the important factor in the localization.

In *Series VIII.* we attempted to ascertain the location of the

eight points in the equatorial circle in the subjective auditory field of the subject *A*. Starting from a point on the equatorial circle about 45° from the point whose subjective location was required, the telephone was moved slowly toward the latter point, until the subject declared that it had been reached. The distance between the subjective and objective location of the point was then measured. Five measurements were made from each side of the point, and the mean of the averages of the two sets of five was chosen as the subjective location of the point. The locations were found to be as follows: *f*, 8.2° toward *r*;

FIG. 1.



The dotted lines intersecting the outer circle indicate the places at which the subject localized the cardinal points on the equatorial circle. (For explanation, see Series VIII.)

The arcs of the incomplete inner circle indicate roughly the limits of variation in the subjective field of the sounds localized at the several cardinal points opposite the arcs. Thus, a sound localized at 90° was almost invariably felt as coming from a point somewhat back of 90°, although the amount of this subjective displacement varied, as the line shows.

fr, 2.6° toward *f*; *r*, 9.4° toward *f*; *br*, 2.2° toward *b*; *b* at *b*; *bl*, 12.5° toward *b*; *l*, 16° toward *f*; *fl*, 10.1° toward *f*. The mean variations for the sets of five were, in degrees, as follows: at *f* 2.2; at *fr* 2.5; at *r* 2.7; at *br* 2.7; at *b* 3.4; at *bl* 4.8; at *l* 2.6; at *fl* 1.7.

The accompanying diagram illustrates the facts brought out in this series, together with certain other peculiarities of the subjective field of auditory localization.

Series IX. has already been adequately described.

The remaining six series were for the purpose of testing definitely the hypothesis that *A's* localization of sounds rested upon the modification, through the shape of the ear, head or body, of particular partial tones for particular directions. The hypothesis will be stated more fully later. Assuming it to be true, we should expect the accuracy of localization to vary with the complexity of the sound, with the pitch, and possibly with the distance. Accordingly, the remainder of the experiments was devoted to testing these points.

In *Series X.* we used the Galton whistle in place of the telephone. The tone employed was of approximately 10,700 vibrations per second. 92 positions were given, of which 29, or 31.5%, were localized correctly. From the median plane 3 were transposed to the left, 8 to the right; from the left 8 were transposed to the median plane, 3 to the right; from the right 3 were transposed to the median plane.

In *Series XI.* a tone of approximately 5,000 vibrations per second was used. 62 positions of the sound were given, of which 14, or 22.6%, were localized correctly. The transpositions were: from middle to left 6, to right 9; from left to middle 2; from right to middle 3.

In *Series XII.* the whistle tone was lowered to approximately 2,500 vibrations per second. 46 positions were given, of which 32, or 70%, were correctly localized; no new transpositions appeared.

The tone of the Galton whistle is not pure, but save in the case of the deeper tones which it is capable of giving, *e. g.*, that used in Series XII., the partial tones are weak and very high. The complex nature of the tone is, therefore, not easily

detected. We should expect, then, that the localizations of its higher tones would be more uncertain than those of the telephone. A reference to Series X. and XI. shows this to be the fact. Comparison may be made with either Series I. or V.

We should expect Series X. to show a smaller percentage of correct localizations than Series XI., as qualitative differences ought to be more difficult of detection, because the partial tones should be higher and weaker. The percentage of correct localizations is, however, slightly in favor of Series X. This fact is more than offset by the nature of the confusions and transpositions which are encountered in Series X. The hemispheres are confused with each other, as well as with the median plane. This does not occur in Series XI. The total number of tests is too small to warrant great insistence upon this difference. But the marked difference between Series X. and XI. on the one hand and Series XII. on the other suggests strikingly the effect of using sounds whose partial tones are weak and very high, as compared with those possessing partials which are relatively strong and well within the limits of audition. Series XII. shows almost as good results as Series V.

In *Series XIII.* we tried to discover whether the peculiar quality of the sound supposed to be the basis of its localization was a function of its distance from the ear. Accordingly, the eight equatorial positions were given each four times at a point thirty inches beyond the surface of the sphere, the telephone being used as the source of sound. The results were the same, however, as the normal results given in Series V. We can hardly conclude from this that greater variations of distance would be equally ineffective.

In *Series XIV.* the sound was given with a tuning-fork of 1,024 vibrations. The fork was placed at the surface of the sphere and only the eight equatorial positions were given. 42 sounds were given, of which 6, or 14.3 %, were correctly localized; the latter were one each at *f*, *fr*, *l* and *fl*, and two at *r*. In 10 cases the subject was unable to localize the sound at all.

In *Series XV.* we substituted a fork of 512 vibrations. 24 sounds were given, of which none were correctly localized;

20 were incorrectly localized and 4 could not be localized. Similar results were obtained with a fork of 256 vibrations.

The tuning-fork tones of Series XIV. and XV. are theoretically pure tones. As a matter of fact it is difficult to avoid, in striking the forks, some slight production of partial tones. This is more noticeable with the 1,024 fork than with the 512 and 256. For the two lower forks localization is clearly out of the question. In the case of the 1,024 fork, also, it is essentially impossible. The number of localizations correctly made in the latter case is somewhat above the number to be expected by the laws of chance, but it is to be added that the subjective attitude even in these correct cases was one of extreme uncertainty, and the total number of tests made is too small to permit of attaching much importance to the operations of mere chance. With the tuning-forks, and in less degree with all of the sounds we used, the change in position is accompanied by slight apparent changes in pitch. In the case of the telephone and whistle tests this change is partially, at least, incidental to the modification of the partial tones. In the case of the tuning-forks the cause is probably of another character, but whatever its nature it is inadequate to afford reliable localizations under the conditions we have studied.

III.

Taking account of all the facts brought out by the tests, we reach the following conclusions:

1. The differences in the localizing capacity for complex sounds in binaural and monaural hearing are, so far as concerns these subjects, interpretable as chiefly differences in the magnitude of the difference limen for locality, rather than as absolute differences in the kind of localizing process involved. In monaural hearing the localization of sounds is by no means a matter of chance. On the side of the intact ear the localizations are in no sense seriously defective, and the distinction between front and back may be made in a manner superior to that of binaural hearing. (Cf. Series I., II. and V.) In the region opposite the deaf ear the localizations are extremely uncertain, but

elsewhere the errors met with are simply slight exaggerations of those made in binaural hearing.

It is not intended to imply in this statement that monaural localization is generally as prompt as binaural, nor that it is as accurate in its capacities for dealing with new and strange sounds. (Compare Series I. with Series II. and V. showing practice effects.) But practically all complex sounds are localized with a reasonable degree of accuracy, subject to the exceptions mentioned in connection with Series X., XI., XII., even upon a first hearing, and a little experience with the sound, familiarizing the subject with its quality changes, leads to such accuracy in its localization as is exhibited in Series V.

2. The experiments objectively considered amply sustain the introspection of the subject in pointing to qualitative differences in the sounds coming from different directions, as the basis of the localizations. This was explicitly commented upon by the subject very early in the course of the experimentation.

Sounds which are complex in nature undoubtedly undergo modification through the damping and reinforcing of their partial tones by the pinnæ, the external meatus and the head, in a manner which must vary somewhat regularly with variations in the spatial position of the object from which the sound emanates. When the changes in these objective positions are small, the corresponding changes in the quality of the sounds are ordinarily minute. But there is undoubtedly a general parallelism between the two series of events, and there is no reason why these variations should not be employed for auditory localizations as genuinely in monaural hearing as in binaural hearing, albeit necessarily with less of accuracy in some portions of the field.

The one region throughout which in monaural hearing such differences in the quality of sounds would be absent, or seriously ambiguous, is that immediately opposite the defective ear. In this case the whole of the head is interposed between the source of the sound and the intact ear. Sounds are much weakened in absolute intensity by this circumstance, as anyone may speedily convince himself, and the damping of the partial tones, upon which depend the qualitative peculiarities of sounds,

is essentially similar for all points in the region.[1] The extreme uncertainty and the persistent confusions which we encounter in monaural localization throughout this region are then precisely what we should anticipate, provided the qualitative differences of sounds constitute the basis of such localization.

Certainly it is striking that those sounds are most accurately and most promptly localized, which contain a considerable number of partial tones well within the limits of audition. The more nearly the sounds approach pure tones, the more inaccurate the localization. This is true regardless of the absolute pitch. Genuinely pure tones are essentially unlocalizable in monaural hearing. Such localization of these tones as is possible in binaural hearing is, therefore, probably referable to the intensity factor.[2] It is to be remembered that the deeper tones which we employed are practically pure. Most of the higher tones possess a few overtones. The sounds most accurately localized were always those in which qualitative differences were noticed and in which modification of partial tones was physically possible under the conditions.

It is assumed, in this statement, that the effects of experience, involving especially motor and visual processes, enter indispensably into acts of auditory localization. With adults, these intensive-qualitative auditory differences, to which we have made constant reference, simply furnish the relatively fixed psychic contents, whose spatial relations can then be learned. The process of giving to these elements values in the general visual-tactual-kinæsthetic space world is definitely the business of experience. How far some of these motor processes may be connected in a genuinely reflex and hereditary way with auditory stimulations is not a matter which we can at present discuss.

[1] The failure of some persons deaf in one ear to localize with any considerable degree of accuracy may be due to any one of a number of reasons, no one of which would impeach the validity of our interpretation.

[2] This radical defect in the monaural localization of pure tones furnishes a serious difficulty to those theories which refer auditory localization to the reflex action of the semi-circular canals. One can conceive of reasons why, as in binaural hearing, such localization should be uncertain and relatively inaccurate. But it is not so easy to explain how in monaural localization the process should work fairly well for many sounds, only to break down hopelessly upon pure tones.

Our examination of monaural hearing suggests, however, the extent to which one set of auditory symbols, *i. e.*, qualitative peculiarities in sounds, may serve for localizations. It substantiates indirectly, therefore, those theories of binaural localization which emphasize quality changes as more important for consciousness than mere variations in intensity.

It may be suggested that the changes in the quality of sounds, which seem to furnish the basis of monaural localization, are in one particular seriously ambiguous. Changes in distance, as well as changes in direction, are frequent causes of an apparent change in auditory quality. This being true, it would seem to indicate that in monaural hearing these two changes would often be confused with each other.

Undoubtedly variations in the quality of sounds may be produced by changes of either kind. But it is by no means certain that the effects upon the quality of the two kinds of change are sufficiently similar to be often confusing. Certainly our tests in Series XIII., although by no means conclusive, suggest strongly the ability to distinguish such qualitative-intensive differences as are due to changes in direction, from such as are due to changes of distance.

3. The presence of eye-reflexes was often very marked, and the final localization was frequently made on the basis of a seeming correspondence between the eye-strains and the supposed direction of the sound. This statement leaves wholly untouched the physiological basis of the eye-movements. It may be added, however, that the subject is rather a vivid visualizer and almost always thinks of spatial relations in visual form.

4. There is no good evidence for supposing that cutaneous sensations played any part in the localizations.

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CONTRIBUTIONS FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

FURTHER OBSERVATIONS ON THE MONAURAL LOCALIZATION OF SOUND.

BY PROFESSOR JAMES ROWLAND ANGELL AND DR. WARNER FITE.

The present paper is a sequel to one published in the May number of the REVIEW for the current year. In the latter we reported an extended series of experiments upon a man totally deaf in one ear, showing that he possessed a somewhat surprising ability to locate certain kinds of sounds, *i. e.*, complex sounds of medium tonal range, although entirely unable to locate pure tones. Without attempting to restate in detail the conclusions we felt warranted in drawing from our observations, it may suffice to say that we connected the subject's localizing capacity with the modifications of timbre which complex sounds undergo when they come to the ear from different directions. These modifications are due to the damping or reinforcing of their constituent partials by the pinna, the external meatus, the bones of the head, etc. The present report is offered as a supplement confirming the main thesis of the previous paper by investigations upon other subjects. It brings out with special clearness the variations in monaural localization which are connected with the duration of deafness. The cases afford from this point of view a somewhat striking explanation of the contradictory reports emanating from aurists with reference to the effects of unilateral deafness upon sound localization.

Like case *A* of the earlier paper, these new cases are all

instances of internal ear deafness. We shall designate them as *C*, *D*, *E* and *F*.[1] They display in a striking manner the differences between the localizing capacity of persons who have recently lost their hearing in one ear and those who have been deaf long enough to accommodate themselves to the abnormal conditions. It does not follow from this that all persons deaf in one ear necessarily develop their latent capacities of monaural localization, and it seems probable that this is less likely to occur when deafness has come on in mature life than when it exists from early childhood. All our subjects, however, have shown ability to improve somewhat rapidly under the training afforded by the experimentation, although they have, of course, been kept in entire ignorance of the correctness or incorrectness of their judgments during the course of the tests.[2] *C* is a man of thirty, who has been deaf from early childhood. The precise time and conditions of the onset of deafness cannot be verified. The subject is under the impression that the trouble followed a

[1] We regret that Dr. Politzer's paper on 'Paracusis Loci' (*Archiv für Ohrenheilkunde*, Bd. XI., p. 231), which is the only account of extended observations on monaural localization to which we have had access, does not enter into more of detail concerning the exact methods he employed and the clinical features of the cases. These were in the main apparently instances of middle ear deafness with various degrees of defect in the hearing of one or both ears. It seems improbable that in such cases habituation to the abnormal condition, in contrast to the usual binaural capacities, should have become thoroughly established, as our tests show it may after a considerable period of unilateral total deafness. Dr. Politzer appears to have used as sounds principally the ticking of a watch and whispering. Our tests show that both of these sounds can be localized with considerable accuracy monaurally, the whispering better than the ticking. But they are less easily located than many other sounds, owing to the peculiarities of their physical composition. These peculiarities of various kinds of acoustic stimuli are more fully discussed in our first paper. This statement will, however, explain our inability to compare our results more explicitly with Dr. Politzer's.

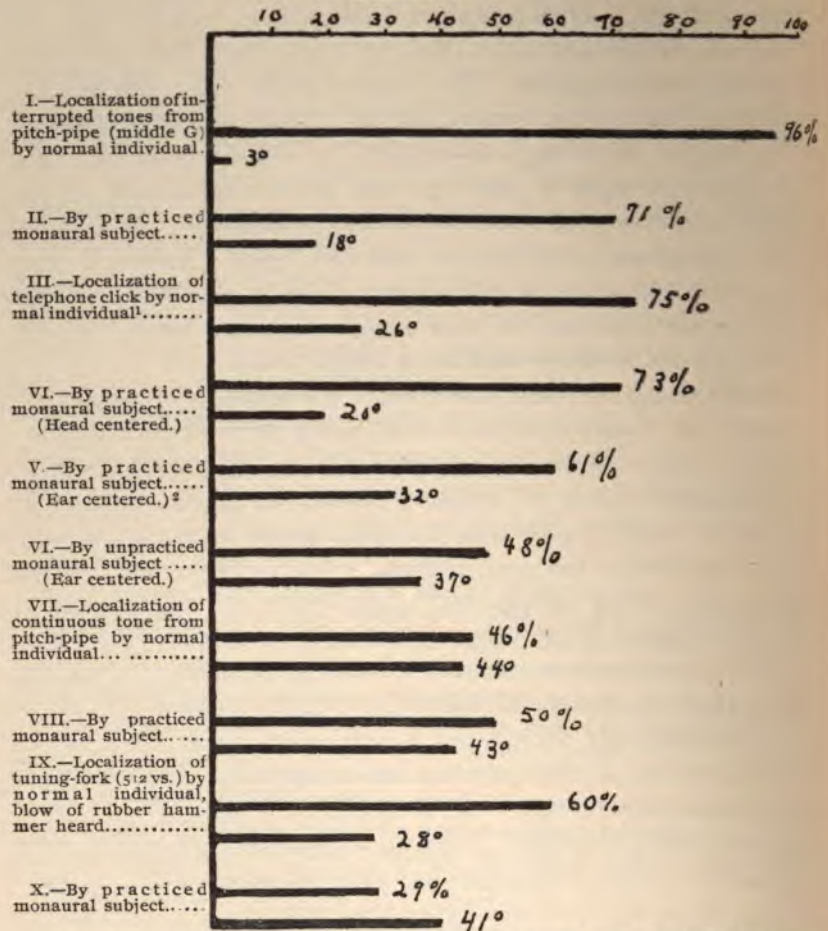
[2] The apparatus employed is described in the first paper. In the experiments recorded here the subject had the head placed at the center of the spherical cage and not the intact ear. Our observations upon *A* seemed to indicate that localization occurred with reference to the whole body and not with reference primarily to the ear. The variations due to alteration of the position in this particular are small and we have not felt warranted in taking the additional time necessary to test the matter conclusively, although the gentlemen who have served as subjects for us have been most generous in their sacrifice of time and convenience.

febrile disease. In later childhood there were transient attacks of severe vertigo. *D* is a man of twenty-four, who lost the use of one ear when ten years old, in connection with an acute attack of osteo-myelitis. *E* is twenty years old. He lost the hearing of one ear six years ago as a result of hemorrhage in the labyrinth following a severe fall. *F* is sixty years old. He was operated upon a year ago for acute suppuration of the mastoid accompanied with labyrinthine symptoms. At the time of the experiments the wound was still open. Taken in connection with *A*'s case, already reported, this gives us observations upon two persons deaf at least twenty-six years, one ten years, one six years, and one deaf for a single year.

When compared with one another the results show increased capacity of localization with increasing periods of deafness. The new cases show precisely the same relative accuracy with the different forms of auditory stimuli employed as did *A*. The more nearly a pure tone is approximated as a stimulus, the more inaccurate the localization. An improvement in the method of giving tuning-fork tones, which was also employed with *A*, resulted in securing a practically pure tone without any of the noise elements incident to setting the fork in vibration. Under these conditions all the subjects failed uniformly to localize. Occasionally such tones would seem opposite the intact ear, but quite as often no location could be assigned, and it seems quite safe to say that in monaural hearing really pure tones are unlocalizable.[1] Very high-pitched tones like those of the Galton whistle are subject to rather large errors of localiza-

[1] The earlier tests on *A*, employing tuning-forks, had been made with as perfect suppression of the composite sounds accompanying the starting of the vibrations as the means at our disposal seemed to permit. We were aware, however, that the result was not wholly satisfactory, and that we did not always secure the purity of tone desiderated. The results of the tests confirmed this in a demonstrable inability to localize most of the tones, combined with an anomalous certainty and accuracy in the localization of occasional sounds of this type. The method finally employed involves having the subject stop the intact ear with the finger until after the fork has been struck and all the overtones thus produced have died away. He is then touched with a long rod by the operator in such a manner as to convey no indication of the direction of the operator or the fork. Upon this signal he opens the ear and attempts to localize the sound. Under these conditions the localization appears impossible, regardless of the intensity of the tone.

TABLE I.



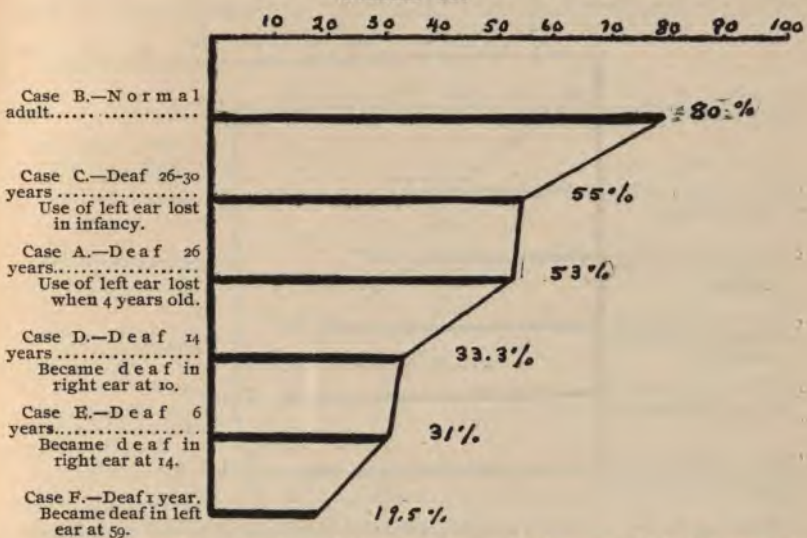
Showing comparative accuracy in auditory localization of a normal person and a practiced monaural subject. Entry VI. introduces for additional comparison an unpracticed monaural subject. Of each pair of lines the upper one represents the percentage of correct judgments, the lower one showing the average error in degrees. The tests upon which the table is based involve the localization of sixteen points (eight on the equatorial circle, four above and four below), as explained in the May number of this REVIEW (Vol. VIII., p. 232). 1,449 tests are incorporated in the table, of which approximately one-fourth were made upon normal individuals.

[1] Confusions between front and back (180°), give the average error in this case a somewhat misleading magnitude. Apart from this confusion, the errors are generally small.

[2] In this case the number showing the average error (32°), is approximate and not exact. A wayward record sheet prevents accurate verification. The possible error is small.

tion for reasons canvassed in our first paper. Complex noises like the rapid clickings of the telephone plate are localized with considerable accuracy. Similarly, complex tones near the middle of the musical scale are well localized. An ordinary reed pitch-pipe (middle G) was employed for the purpose.[1] In the case of this complex tone it proved, as in binaural hearing, considerably more difficult to effect a confident localization when the tone was continuous than when it was interrupted. Not to mention other possible reasons for this, it is certain that the identification of the tonal modifications, which are in monaural

TABLE II.


10 20 30 40 50 60 70 80 90 100
Case B.—Normal adult.....
80 %
Case C.—Deaf 26-30 years.....
Use of left ear lost in infancy.
55 %
Case A.—Deaf 26 years.....
Use of left ear lost when 4 years old.
53 %
Case D.—Deaf 14 years.....
Became deaf in right ear at 10.
33.3%
Case E.—Deaf 6 years.....
Became deaf in right ear at 14.
31%
Case F.—Deaf 1 year.
Became deaf in left ear at 59.
19.5 %

Showing by the percentage of correct judgments the variation in the capacity of monaural localization of the telephone click (eight points on the equatorial circle), as connected with the duration of deafness.

hearing the indices of direction, is much assisted by rather rapid repetitions of the tone.

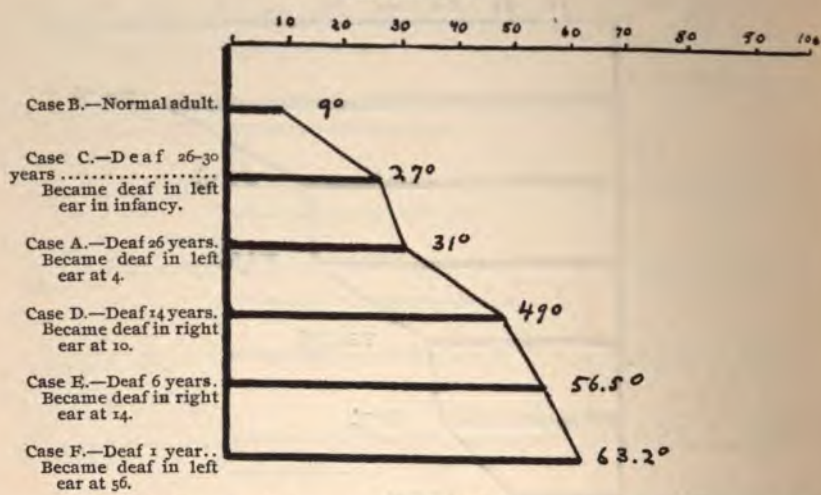
It does not seem essential to present a complete tabular statement, such as was given in the report of *A*'s case, for each form of test with the four new subjects. The differences are purely matters of degree, which can be exhibited satisfactorily in a briefer way. We have come upon no new principle, nor have

[1] This series, in the case of *A*, was not contained in the original report, but his results with this stimulus fall in perfectly with our general hypothesis.

we failed in any case to verify the hypothesis we have offered. We may first represent graphically the differences in the capacity of localization between a well-trained monaural subject like *A* and a normal person, when sounds of varying complexity are used as stimuli. Table I. exhibits these relations.

Table II. presents the variations among persons deaf in one ear, as connected with the duration of their deafness and the period at which they became deaf. The percentage of correct judgments is used as the basis of the table, the same sound, a telephone click, being used throughout the tests.

TABLE III.



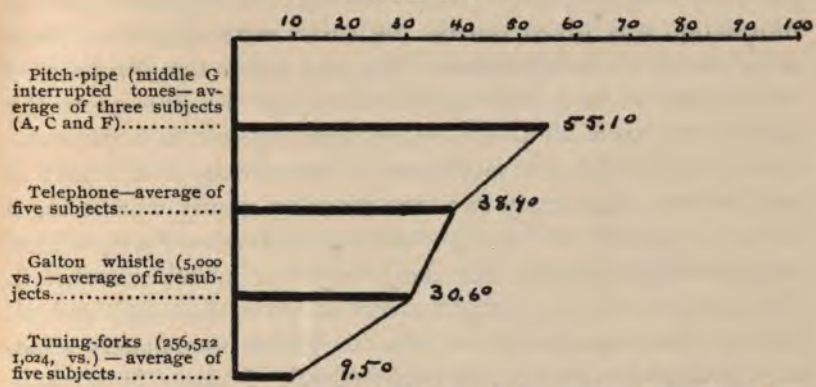
Showing in degrees the average amount of error made by the several subjects in attempting to localize eight points upon the equatorial circle. Telephone click used as stimulus.

Table III. shows the same general relations, but the average error in degrees is made the basis of the comparison, instead of the percentage of correct localizations. Table III. is probably more truly indicative of the differences between monaural and binaural hearing, and of the variations among monaural individuals themselves, than Table II.

In Table IV. we have shown graphically, on the basis of results from all the monaural cases we have examined, the variation in accuracy of localization, as compared with the noticeable

complexity in the sound. The fact should be emphasized, that all the subjects agree in their judgment of the relative ease of localization of the general kinds of stimuli, that, furthermore, their subjective impressions are verified by their objective accuracy, and that the diagram is, therefore, representative of an average, which does no violence to the relations in any individual case, as so often occurs in averages based on statistical evidence. The tuning-fork tests are included for tabular completeness only. It is perfectly certain, we think, that pure tones are unlocalizable in monaural hearing. Mere chance might be expected to produce a larger percentage of correct judgments than is here presented. But neither the percentage test nor the

TABLE IV.


10 20 30 40 50 60 70 80 90 100
Pitch-pipe (middle G interrupted tones—average of three subjects (A, C and F).....
55.1°
Telephone—average of five subjects.....
38.4°
Galton whistle (5,000 vs.)—average of five subjects.....
30.6°
Tuning-forks (256,512 1,024, vs.) — average of five subjects. .....
9.5°

Showing by percentage of correct judgments for various forms of auditory stimuli the connection between accuracy of localization and discriminable complexity of sounds (eight points on equatorial circle).

average error test is to be weighed beside the overwhelming deliverance of consciousness itself, which in the case of pure tones is absolutely different from that in the case of complex sounds.

So many of the aurists' tests are made with a watch and by means of whispering (this is true in Politzer's tests already referred to), that in view of the frequently asserted inability of monaural subjects to localize those sounds, it seems desirable to refer briefly to our own tests, which show clearly enough that such sounds, although more difficult to localize than some others, are still capable of decidedly accurate localization. The

occasional inability to localize is, therefore, probably to be connected with conditions of aural irritation, or with the lack of adaptation to the new monaural conditions.

Our tests were made in a manner already suggested in the description of the procedure in the experiments with tuning-forks. The operator, having removed his shoes, took his position at any desired distance from the subject (as a matter of fact we used various distances) and in any desired direction from him. The subject during this period kept the intact ear tightly closed. On being touched with the signal rod, he opened the ear and attempted to localize the sound. As the operator moved noiselessly, it was also possible to make tests without the stoppage of the ear. No suggestion was obtained in any case of the direction from which the sound might be expected to come, and no information was given during the series regarding the success or failure of the localization. We may quote the results of a single series of tests with each kind of sound. We quote the first tests of each kind, which represent, therefore, a minimum of familiarity with the sound and a maximum of difficulty in localization. Of twenty-two tests made by gently whispering a monosyllable like 'K' at eight different points on the equatorial circle (average distance four feet), only four mistakes occurred. They involved in no case an error of more than 45° and all four of the confusions were between points immediately opposite the deaf ear and points slightly back of that. If the confusion between front and back, which is so frequent in binaural hearing, be taken into account, this result is quite as accurate as most normal individuals would attain on a first trial. It certainly is difficult to reconcile it with any radical inability to localize.

Similarly, of the first series of tests with a watch (distance two feet) ten out of sixteen positions were correctly judged, the same points being used as in the previous test. No error was larger than 45°, and an error of this amount is frequently met with in first tests upon normal individuals, as our records show. Decidedly the watch must be used sparingly, if accurate localization is desired with either binaural or monaural persons.

In series XIII. of our earlier paper we attempted under the

limitations of indoor work to decide whether the changes in the character of complex sounds, furnishing the basis of determining their direction, were functions of changes of distance also. The indoor tests resulted negatively, the subject showing highly accurate capacity to distinguish changes of distance from those of direction, thereby demonstrating that, within the limits of variation possible under the laboratory conditions, the two series of changes need not lead to serious confusions. It seems judicious to report at this time a more drastic test made out of doors. We may anticipate the result at once, however, in saying that the test failed to show any great difficulty in distinguishing distance changes from changes of direction.

The tests were made at night, with the greatest attainable quiet, in a large open field some eight or ten acres in area. Precautions similar to those adopted in the whispering test were introduced to prevent any suggestion of the direction from which the sound might be expected. We used the same eight directions employed all through our tests, varying in entirely irregular order the distances from five to twenty-five and fifty yards. The sounds might thus come from points on any one of three concentric circles, and the subject was required to judge both the distance and the direction. As stimuli we employed the pitch-pipe previously used, and an automatic counter, resembling a pedometer in its mechanism, and producing when operated rapidly a clear, metallic, clicking sound. Indoor tests had shown that both sounds could be localized very accurately. Seventy-two tests were made, in which the direction was correctly judged 52 times, or 72 %. The average error was 14.3°, and only two errors were made in excess of 45°. These both involved the point immediately opposite the deaf ear. Of the 20 erroneous judgments, 4 also involved errors of distance. In addition to this there occurred 5 errors of distance without errors of direction. All the errors of distance were caused by confusions of sounds 25 yards distant with those 50 yards distant. When it is remembered that at the greater distances the sounds were only with difficulty audible on the side of the deaf ear, this confusion is not remarkable. On the whole, therefore, the test seems to confirm the indoor experiments in its indication that with com-

plex sounds there are relatively few confusions of distance and direction, the modifications which the sounds undergo by changes of the two kinds being differentiable in the main by the single ear. Sounds near the threshold of intensity are, as is well known, very difficult to localize in normal hearing, as well as in monaural hearing. We should expect to meet with a much larger percentage of confusions, therefore, if we employed distances so great that the sounds could barely be heard in any direction. A similar result would accrue from intrinsically feeble sounds near at hand. This is true with the watch tick, for example.

The experiments suggest very pointedly the possibility and desirability of a little training for persons who lose their hearing in one ear. If the disaster occurs in infancy or early childhood, it seems probable from our observations that experience itself serves to build up the new localizing processes. But when the injury occurs later on in life, it appears problematic how far the adaptation is carried. In any case it is well recognized that many patients suffer considerable distress through their lack of confidence in their capacity to localize sounds, and it seems probable that this distress can be largely removed by a little formal drill, by means of which the patient may have opportunity to notice carefully the changes in sounds arising from changes in their position. The most defective case we have examined (*F*) showed an increase after one hour's practice from 12.5% of correct localizations to 25%, and a decrease of his average error in degrees from 70° to 53°. This improvement occurred, too, without the subject's knowing whether his judgments were correct or incorrect. The result is attributable, therefore, merely to systematic direction of attention to the peculiarities of sounds coming from different directions. If to this were added the knowledge from visual sources of the position of the stimulus, there is every reason to anticipate a very great and rapid improvement with considerable, if not complete, relief from the distress already mentioned. The gaining of self-confidence in the matter is a large part of the victory. The discipline suggested is distinctly comparable with the ocular gymnastics prescribed to persons suffering from defects of the oculomotor mechanism, and is herewith respectfully submitted to the attention of aurists who may not employ such methods.

CONTRIBUTIONS FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

New Apparatus.

BY PROFESSOR JAMES ROWLAND ANGELL AND DR. WARNER FITE.

The following description is offered of several pieces of apparatus, which have been found to be of practical value in the psychological laboratory of the University of Chicago.

1. A multiple contact key. This is shown in Plate I. It is intended to eliminate the difficulty of adjustment and the possible source of error found in the use of the common type of bar-key. In the keys used in the Chicago laboratory it was found that the force required to ensure double contact was liable to loosen the screws by which the adjustment for simultaneity was made, also that the bar was liable to bend slightly, giving contacts which were not exactly simultaneous. The error was occasionally found to amount to 10σ or more. No doubt these errors can be overcome by accurate workmanship. But even granting this, the extreme accuracy and the ease of adjustment of the key herewith described lead us to regard it as preferable to the bar form of key.

The key presented here consists of a cylinder of wood (length 23.5 cm., diameter 15 cm.) mounted upon steel supports, which rest upon a wooden base. Upon the surface of the cylinder are fastened six plates of brass, 25 mm. in width, and in length equal to one-half the circumference of the sphere. The thickness is 3 mm., beveled, however, to an edge at each end. They are arranged alternately along the length of the cylinder at intervals of 12 mm. where their edges rest against a straight line drawn parallel to the axis of the cylinder. Resting firmly against the cylinder are six pairs of strips of phosphor-bronze (spring-metal) which constitute the electrodes of the six

electric circuits. Each strip is 2 mm. thick, 9 mm. wide, about 8 cm. high from the platform, and curved slightly at the top, so that only the edge rests against the cylinder. The members of each pair are 6 mm. apart. Each strip is set into a circular bar of brass 25 mm. long and 9 mm. in diameter, through which there is bored a hole with a binding-screw for making connections; this bar is then continued as a screw, which passes through the projecting edge of the base and is secured to the platform by nuts above and below. This arrangement allows the strips to be raised and lowered at will. Each circuit is made when the strips rest against the brass plates of the cylinder, and broken when they rest against the wood. The key thus allows three circuits to be made and three to be broken at the same moment. Simultaneity is secured by adjusting the electrodes so that the lower edge of the pair whose circuit is to be made, rests against the upper edge of the brass plate on the line where the circuits are made and broken, and so that the upper edge of the pair whose circuit is to be broken, rests against the lower edge of its corresponding brass plate on the same line. If the electrodes were filed to a sharp edge the edges might all rest upon the same line, but this was thought to be inadvisable owing to the danger of wear and the consequent disturbance of adjustment. The width of the edge renders it also necessary to confine the adjustment to one of the two lines where the brass plates end, but this is all that the use of the key requires. The simultaneous making and breaking of the several circuits is effected by turning the cylinder, in the direction of a clock, by a rapid wrist-motion, through a semicircle; when the reaction has been made the key is turned, still in the clock-direction, back to its original position.

The key was tested on a drum, after being easily adjusted by the eye, and no error could be found. Whatever the error, it was certainly under .0001 of a second; and whatever error there may be in the adjustment, is minimized by the rapid motion of the surface of the cylinder, which, again, is due to its size. The only improvement we have to suggest is that, where the electrodes are passed through the platform, they be cut square instead of round, so as not to permit of rotation during

Angell and Fite on New Apparatus.

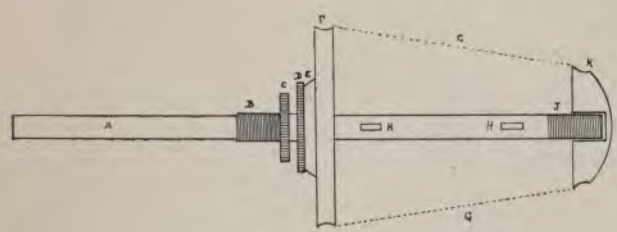
adjustment. Since the accuracy of the key after adjustment depends upon the stability of form of the parts, great care must be taken, in the construction of the cylinder and the base, to secure well-seasoned wood.

II. A platform for tuning-fork and marker. This is shown in Plate I., attached to a Scripture drum. It is intended to facilitate the adjustment of the several forks and markers to the drum, particularly to enable them all to be withdrawn or applied in one movement. The fork and marker rest upon a steel platform (25 cm. x 7.5 cm.) down the middle line of which is cut a groove, 20 mm. wide, with projecting flanges. The fork is secured by two screwbolts, the heads of which slide in the groove, with thumbscrews on the upper end, as shown in the photograph plate. The marker is secured to a rod 10 mm. in diameter. The rod ends below in a bolthead which slides in the groove, and is clamped to the platform by a thumbscrew. The fork and marker may thus be located anywhere along the length of the platform. This upper platform is screwed to a steel plate (100 x 80 x 5 mm.) which moves in a track to and from the drum upon a lower platform. The latter ends on the right in a socket, which fits over an upright rod (diameter 19 mm.) and thus fixes the whole apparatus to the platform of the drum. When the fork and marker have been once adjusted to the drum their relative position remains fixed. By means of the screw which appears at the front of the platform, they may, with the upper platform, be withdrawn from the drum and re-applied without any alteration of relative adjustment. The fork can thus be easily set in vibration before coming into contact with the drum. The head of the adjusting screw can easily be supplied with a circular vernier, if extreme delicacy and accuracy of adjustment are found necessary.

III. An apparatus for determining the impact limen is shown on the lower portion of Plate II. In the use of cork weights great difficulty is always experienced in placing the stimulus gently and steadily upon the skin. The instrument shown in the plate employs the principle of the balance to secure this result. It is simply a very delicate balance with three knife-edge

bearings, weighing accurately down to a milligram, mounted on a heavy steel platform raised on levelling-screws and provided with tracks at right angles to each other, upon which it travels by means of screws. In this way a considerable area of the skin can be explored without disturbance to the subject or the instrument. The mode of operation is this: The long arm at the left of the instrument carries a flat cork point, which can be detached by a screw and replaced by another of any given shape or dimensions. The cork point is attached to a flat bar sliding up into a slot in the arm. This permits a vernier arrangement, as shown on the plate. The sliding bar carrying the point is clamped firmly at any desired point by a friction-clutch, which may be seen projecting toward the body of the instrument. When the arm, supposing this to be the portion of the skin to be explored, is properly placed, the cork point is lowered, until it is just in contact with the skin, when the pointer of the balance is opposite to zero on the dial. The point is then pushed up any desired amount, say two millimeters. A weight of any desired size (for example, 10 milligrams) is placed in the pan over the arm. At the base of the support for the balance-arms are seen two levers. One of these releases the balance and permits the cork point to move gently down upon the skin. The oscillations are very slow and as the point comes into contact with the skin the balance-arm moves up again and the second lever is then pressed down, bringing the pointer to zero once more. The height of the fall and the weight in the pan can either or both be varied, and the physical expression for any given stimulus is easily computed. The instrument is noiseless and capable of rapid manipulation.[1] It must be constructed with great delicacy to secure constant action in the oscillations of the arms, and these oscillations must be slow to secure the best results. In two years and more of use it has proved very reliable. It can readily be converted into an instrument for giving passive pressure stimulations instead of impact. The principal dimensions are as follows: base plates 25 cm. square, balance arm 30 cm. long, supporting pillar and contact arm, when pushed in, each 15 cm. long.

[1] Cf. Griffing's lucid presentation of relevant considerations in his monograph, 'Sensations from Pressure and Impact,' 1896.



Angell and Fite on New Apparatus.

IV. A standard for adjusting a common form of the registering tambour to the surface of the drum in plethysmographic work is shown on the upper portion of Plate II. It consists of a heavy metal base carrying an adjustable upright, permitting the raising or lowering of the carriage above by means of the screw-clamp upon which, in the plate, rests the glass tube. The carriage itself is adjustable around a horizontal axis by means of a thumbscrew. It travels forward and backward by means of a track and an endless screw of fine thread. The clamp mounted upon the carriage to receive the rod of the tambour is adjustable around a vertical axis. For delicate work some such device is almost indispensable and this is as convenient as any form we have seen for tambours of this type. We give no dimensions, as these must necessarily be dependent upon the particular kymograph and tambours to be employed.

V. An adjustable form of the Hallion and Comte plethysmograph. This is shown in the sketch in the middle section of Plate II. *A* is a thin metal tube (length 15 cm.) carrying the metal disk *F* (diameter 6 cm. by 5 mm. in thickness). *A* is perforated with slits at *H* and terminates in the metal disk *K* (diameter 3 cm., thickness 5 mm.). *E* is a leather washer, 5 mm. thick, compressed and held in place by the two metal washers *C* and *D*. *G* shows the position of the rubber covering, which is tied and cemented to the grooved edges of *F* and *K*. The screw-threads at *B* and *J* on *A* permit, by changing the distance between *F* and *K*, an adjustment of the tension of the rubber covering *G*, after the joints of the rims of *F* and *K* have been made air-tight. It is thus possible to secure the maximum efficiency from the instrument without dependence upon the skill with which the rubber chances to be stretched at first. We have found the conical form of the instrument preferable to the cylindrical shape commonly employed. It permits a more perfect contact between the surface of the fingers and the rubber.

VI. Thermal apparatus (shown on Plate III.). This consists of two parts; the first is for the purpose of maintaining a

constant temperature; the second for the purpose of locating points on the skin.

a. The first appears in the lower part of the plate, resting on the larger table. The larger and smaller vessels are for hot and cold water respectively, which passes in a continuous stream to the temperature-point on the smaller table. The problem of keeping the hot water at a constant temperature without the aid of an assistant, is solved by employing a constant flame, a constant quantity of water and a constant supply of fresh water at an approximately constant temperature. The first can usually be secured by a group of Bunsen burners; to secure the last two we used a tank of galvanized iron (diameter 35 cm., height 50 cm.), tight at the top with the exception of a hole (diameter 4 cm.) into which as a gauge we fitted a glass tube in a cork. The tank is always kept full, its fulness being indicated by the presence of water in the glass tube, which is open at the top. In order to prevent the tube from overflowing, also to avoid undue pressure within the tank, the water which comes through the feed-tube to the tank is not drawn directly from the faucet, but from a bottle with an opening near the bottom (an aspirator), which is set in a pan upon a shelf above the feed-tube, but not above the top of the tube projecting from the tank. (This part of the apparatus does not appear upon the plate.) The bottle is fed from a faucet through a tube; it is kept constantly full and the overflow is caught by the pan, from an opening at the base of which it is carried off through a rubber tube to a sink. In this manner the pressure of the water flowing through the tank is kept constant. For some purposes it is desirable to have only a small amount of water in the tank. On such occasions the pressure is of course only approximately constant. The flow of water through the apparatus as a whole is regulated by the lower glass stopcock fastened to the standard at the further end of the large table. The water flows from the temperature-point, through this stopcock, to the glass tube fixed to the standard at the rear end of the large table, thence into the funnel and out through the waste-tube to the sink. By allowing the water to fall into the funnel, instead of running out through a continuous circuit of tubing, it is possible to estimate, by the

size of the stream outlined against the black cardboard, the approximate rate of flow. The stopcock thus governs the rate of inflow as well as of outflow; if the flow outward is completely checked, the result is that all the water from the faucet overflows the aspirator into the pan and thence returns to the sink, the quantity of water in the tank remaining always the same; in this manner it is possible easily to regulate the amount of cold water coming in to cool the hot water in the tank. By adjusting the supply of gas and water any given temperature from 34° C. to 60° C. can be secured in the contact point and indefinitely retained. Much higher temperatures can readily be obtained, if necessary. The thermostatic devices employed to render such apparatus automatic can be applied, if desired. But the expense is thereby considerably increased.

By opening and closing the proper tubes with clamps the cold-water tank is brought into the system and the hot water separated from it. The water flowing through the cold-water tank flows not directly into the tank but through a coil of lead-pipe 37 feet in length and thence out to the temperature-point. The tank itself is filled with cracked ice and salt, which cools the water during its passage through the lead-coil. Since the temperature to which the water is reduced depends upon the rate of flow, it can be regulated within certain limits by the use of the stopcock. By varying the amount of ice in the tank and, if necessary, opening the circuit from the hot-water tank, it is easily possible to secure and retain temperatures in the point ranging from + 34° C. to — 12° C. In working with temperatures below the freezing point, alcohol should be mixed with the water supplied to the coil. By substituting a small amount of ether and solid carbon dioxide for the cracked ice and salt, temperatures as low as — 100 C. can be obtained. The temperature at the copper point varies widely in this case with the length of tube, the wrapping, etc., between the point and tank.

Both tanks are heavily jacketed with asbestos, which is not shown in the plate. The cut fails also to show a stopcock for drawing off the melted ice from the cold-water tank. Double tubing is used to connect the point with the cold-water tank. This has not been found necessary with the hot-water system.

self-evident basis for an explanation of the physiological facts concerning localization. Wundt has advanced the idea that tactile nerves are stimulated by the movements of the tympanic membrane and thus contribute to the localization processes. He has made a similar suggestion with reference to stimulations of the tensor tympani muscle.[1] This type of view has been rigorously criticised by Stumpf, who emphasizes, among other difficulties, the undoubted fact that we can correctly localize two simultaneous sounds.[2] E. Mach early suggested the theory that the external ears act as resonators modifying the quality of sounds heard from different directions, and affording thus a criterion of direction.[3] Theories like those of Preyer and Münsterberg have attempted (thus far with limited success) to make the semicircular canals responsible for the physiological phenomena.[4] So far as these latter theories emphasize the release by sound stimulations of quasi-reflex movements of localization, they point to an important and genuine feature of such processes, whether their conception of the physiological mechanism involved be accepted or not.

On the psychological side various factors have been described as contributing to localization: *e. g.*, (1) the immediate consciousness of position in an auditory space; (2) the consciousness of positional relations gained by visual and other supplementary imagery; (3) the consciousness of tendencies to movement on the part of the head and eyes; and (4) apparently, at times, the consciousness of cutaneous sensations from the shell and membrane of the ear and possibly the tensor muscle.[5] Criticism has been much devoted to combating the frequent and careless assertion that we are conscious of the intensity of the

[1] Wundt, 'Grundzüge der physiologischen Psychologie,' Vol. II. (1893), pp. 93 ff.

[2] Stumpf, 'Tonpsychologie,' Vol. II., pp. 53 ff.

[3] Mach, 'Bemerkungen über die Function der Ohrmuschel,' *Archiv für Ohrenheilkunde*, Vol. IX. (1875), p. 72.

[4] *Cf.* Preyer in *Pflüger's Archiv*, Vol. XL., p. 586; Münsterberg, *Beiträge zur Psychologie*, Heft 2, p. 182.

[5] The general psychological question of the reality of an auditory space has been luminously discussed by Stumpf, and in some of its more empirical features is best treated by Pierce. *Cf.* Stumpf, *loc. cit.*, and also 'Ueber den psychologischen Ursprung der Raumvorstellung,' Leipzig (1873); Pierce, *loc. cit.*

sound heard by each ear and that we in this way localize the source of the sound upon the side most intensely stimulated. The fact is, of course, that we are conscious of one sound and one intensity only, and this is referred to some specific spatial position. But the details of the strictly psychological portion of our general problem have been, perhaps, most often honored by neglect. The recent paper by E. A. McC. Gamble is a notable exception.[1]

So long ago as 1875 Lord Rayleigh had made observations upon the localization of tuning-fork tones, which led him to surmise that differences in the quality and timbre of sounds, as heard by the two ears, were of quite as much significance for localization as the mere differences in the intensity of the fundamental tone.[2] In 1879 S. P. Thompson, discussing experiments of his own with the pseudophone (*cf.* the similar observations of Weber, *Berichte der Gesellschaft der Wissenchaften* [Leipzig, 1851], p. 29 — M. and P. Cl.), came to a like conclusion, which he formulated in a later article somewhat more explicitly.[3]

Despite the instructive character of these investigations, it must be admitted that the conditions which were employed are somewhat unnatural, and that in so far, therefore, they jeopardize the scope of the inferences which may be confidently based upon them. In certain of Lord Rayleigh's experiments, for example, two tuning-forks were struck on different sides of an observer, and then, one of them being stopped, the position of the other was estimated. Evidently the conditions produced by sounding two tones and then suddenly subtracting one are very different, both neurally and psychologically, from those arising when a tone is heard as it originates from some single source. In Thompson's interesting experiments an artificial pair of pinnæ were used, enabling an exaggeration of the effects of reflection, etc., produced by the natural pinnæ. That the localization of sounds could in this fashion be disturbed in certain definite ways

[1] Gamble, PSYCHOLOGICAL REVIEW, Vol. IX. (1902), pp. 357–373.

[2] Rayleigh, *Transactions of the Musical Association* (1876); also *Philosophical Magazine* (5), Vol. III. (1877), p. 456.

[3] Thompson, *Philosophical Magazine*, January–June (1882), p. 415; *ibid.* (5), Vol. VIII. (1879), pp. 385–390.

is not surprising, nor is it remarkable that tuning-fork tones should show much less liability to modification in this manner than more complex sounds. But, in the nature of the case, such observations cannot furnish a complete chain of evidence as to the differences characterizing the localization of these various kinds of sounds under normal conditions. Notwithstanding the limitations upon the implication of these experiments, they certainly constitute presumptive evidence in favor of the belief that quality changes are of genuine significance for sound localization, especially when taken in connection with such observations as Mach's,[1] and such mathematical deductions as Lord Rayleigh has made, showing that, save for a few positions, there is an extremely small difference in the intensity of the stimulation of the two ears by the fundamental tone of a sound.[2]

Pierce has demonstrated that localization in the median plane, which is notoriously uncertain and inaccurate, can be vastly improved, when complex sounds are used, by learning to note the modification in tone-color, or timbre, which is connected with different positions in this plane.[3] He has also made tests with organ pipes and tuning-forks, which suggest that auditory judgments of distance are affected by tonal complexity, the richer tones tending to be judged nearer than those more nearly pure.[4] In a paper published in 1865, Mach remarked a contrary fact and promised further communications upon the subject, which I have, however, been unable to find.[5] Bloch made certain observations, which appear to agree with Pierce — the fuller, richer tones being judged nearer.[6] On the other hand, the computations of Grinwis, showing the relative intensity of the components of a complex sound for various

[1] *Loc. cit.;* also 'Bemerkungen über den Raumsinn des Ohres,' *Poggen. Annalen*, Vol. CXXVI. (1865), p. 331; *Sitzungsberichte der Wiener Akademie*, Vol. L. (1864), 'Ueber einige der physiologischen Akustik angehörigen Erscheinungen.'

[2] *Loc. cit.*

[3] *Loc. cit.*, pp. 92 ff.

[4] *Loc. cit.*, pp. 163 ff.

[5] *Poggen. Annalen, loc. cit.*

[6] Bloch, 'Das binaurale Hören,' Wiesbaden, 1893.

distances, furnish a theoretical confirmation of Mach's view.[1] The issue is really somewhat ambiguous. Richer sounds may ordinarily be judged nearer than those more nearly pure. The upper partial tones of a complex sound may be relatively more prominent when the sound is heard from a distance, and still the total sound effect be poorer and less full than when the same sound is heard near at hand.

The extended experiments recently carried out in the Psychological Laboratory of the University of Chicago showed conclusively that persons totally deaf in one ear could localize sounds of sufficient complexity with considerable accuracy, whereas approximately pure tones could not be localized at all.[2] The localizations were evidently based upon the modifications which the partial tones of complex sounds undergo, when the position of the sources of the sounds is changed relative to the ear. The introspective evidence offered by the observers in these tests confirmed perfectly the objective results in pointing to this explanation. It is interesting to note in passing, in connection with Pierce's observations upon median plane localization, to which reference has already been made, that in the Chicago experiments certain of the persons deaf in one ear distinguished front and back with distinctly greater accuracy than the normal subjects. It will be remembered that for normal persons the intensity criterion is for points in the median plane peculiarly ambiguous. But quality differences are relatively reliable, if one has learned to employ them.

The observations reported in this paper constitute an immediate outgrowth of these several previous discoveries and form an effort to begin the systematic investigation of the part played by the partial tones in the localization of sound. The positive outcome of the work thus far is largely methodological in character and bears most immediately upon points (1) and (3) mentioned in the opening paragraph, and indirectly upon point (2). For reasons which will presently appear, the work is necessarily extremely slow, and the results already attained do not

[1] Grinwis, 'Ueber cylindrische Schallwellen,' *Poggen. Annalen*, 1877, Beibl. 8, p. 443.

[2] Angell and Fite, PSYCHOLOGICAL REVIEW, Vol. VIII. (1901), pp. 225, 449.

seem to warrant a more pretentious title than that I have chosen. The probability of unavoidable delay in the completion of the observations is the justification for publication at this time. Moreover, the implication of the experiments successfully executed seems altogether definite and distinctly significant for the theory of auditory localization.

A solution of the problem in hand evidently involves certain indispensable conditions which the apparatus was designed to meet. There must be (1) a series of sounds of controllable intensity, including (2) at least one approximately pure tone. It must be possible (3) to produce these sounds at any position relative to the observer without his previous knowledge of their location. It must be possible (4) to have all the sounds given at equal distances from the observer. It is desirable also (5) that the distance should be capable of variation, although the present paper does not deal with experiments in which changes of this kind were employed. Not least in importance, as dearly bought experience has taught me, is (6) the absolute prevention of the reflection of the sounds. Working under expert assurance that reflection could be eliminated by properly arranged draperies, I wasted much valuable time indoors, with the result that often tuning-fork tones, when opposite one ear, would confidently be localized as opposite the other. My failure may have been wholly due to unskilful devices on my part, but I certainly question very seriously whether experiments with tuning-forks can be satisfactorily carried on save in the open air. With many kinds of sounds this consideration is of minor consequence. I may mention in connection with these indoor experiments the interesting effects of fatigue which were repeatedly apparent. If one ear were fatigued for a tone, and within a few moments both ears were permitted to receive the sound, the latter would often be confidently localized as opposite the unfatigued ear, or sometimes as in the median plane, depending on the degree of the previous fatigue. Thompson, in the paper already mentioned, remarks a similar phenomenon, but much less extreme than in my observations.[1]

[1] *Philosophical Magazine* (5), Vol. XII., p. 351.

Apparatus and Procedure.

To meet the conditions named, work was carried on outdoors on windless nights—a deplorably infrequent circumstance in Chicago—rendering the observations very protracted. A

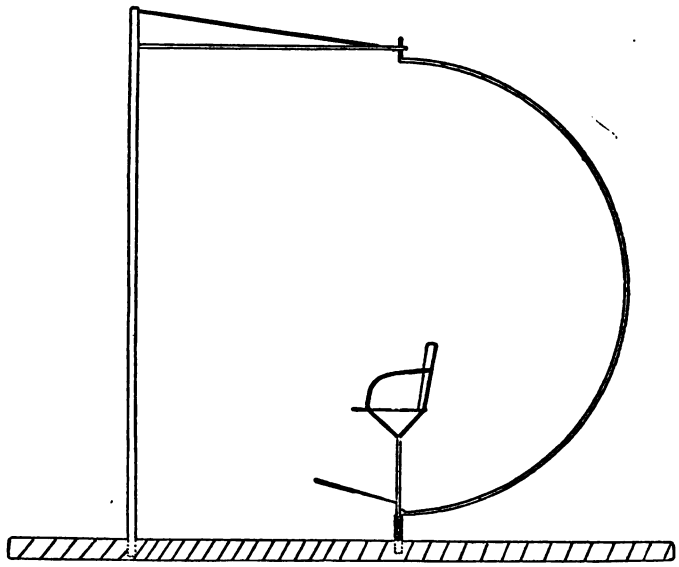


Fig. 1. Platform with Semicircle and Chair.

narrow platform was erected, upon which was mounted the apparatus shown in the accompanying diagrams (Figs. 1 and 2). The upright support (Fig. 1) carries a strong light semicircle

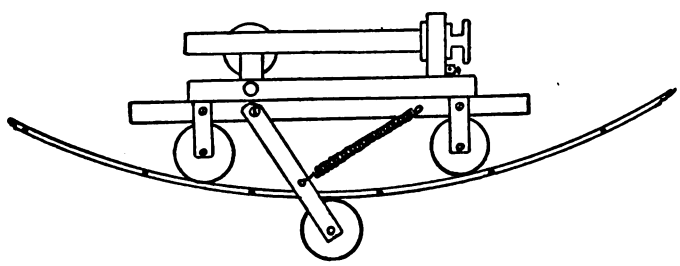


Fig. 2. Carriage with Tuning-fork, Resonator removed.

of steel with a radius of four feet, which is so arranged as to permit a metal carriage mounted on soft rubber rollers (Fig. 2) to travel up and down upon it. The semicircle is marked

off in degrees, so that the position of the carriage can at any time be determined. The carriage is held at any desired height by friction screws acting upon the rollers. The semicircle is very accurately hung and revolves noiselessly. The chair shown in the cut is adjustable in height, and when in use is put at such a point as will bring the line joining the observer's ears into the equatorial plane of the sphere determined by rotating the circle. The chair is mounted on heavy felt cushions which insulate it from any sound-waves which might be transmitted through the semicircle. The platform under the chair is marked off like a compass, so that any position of the circle can be determined. Upon the carriage are fastened the various instruments used for giving the sounds. These are so adjusted that at whatever position the sound is given a constant phase is presented to the observer.

The sounds employed were as follows: From (1) a tuning-fork of 1,000 vs.; (2) a stopped pipe of 768 vs.; (3) a reed pipe of 768 vs.; (4) a bell with a fundamental tone of approximately 2,048 vs.; and (5) a noise made by a telegraphic sounder. It would have been desirable to work with tones which were all of like pitch, but this was out of the question for the time being. It will be observed, however, that all the tones are within the middle range of the musical scale, and that they are quite close together in pitch, two of them being of identical vibration rate. The sounder and bell were operated by closing a noiseless electric contact. The two pipes were controlled by blowing through rubber tubing. The fork requires a somewhat more detailed description. It should be said that the intensity of all the sounds was kept as nearly constant as possible, and that the intensity aimed at was such as to render all of them perfectly distinct, without their becoming unpleasant.

The arrangement for the tuning-fork constituted the most elaborate and most troublesome technical part of the problem. To secure as nearly pure a tone as possible a carefully constructed resonator was made and mounted over the fork. The fork was supplied with a magnet between the tines, and this magnet was then connected with the circuit of an interrupting fork of just one half its own fork's rate of vibration. This is

the device employed by Helmholtz in his celebrated experiments upon vowel sounds.[1] By bridging the spark in the driving fork one secures a tone in the second fork free from all accessory noises of interrupters, hammers, etc. In my experiments the driving fork was kept where it could not be heard, in a house at a distance from the experimental platform. That I thus secured an absolutely pure tone is, perhaps, more than can be confidently asserted. Resonator analysis failed to detect any tone apart from the fundamental, and, so far as concerns my observers, it can be positively stated that they were utterly unable to discern any complexity in the tone. The tone of the stopped pipe was not to them noticeably complex, so that they could confidently detect the overtones, and yet it was not so perfectly pure as the fork. It had the muffled effect characteristic of such tones. All the other sounds were noticeably complex.

My observers sat in an erect position, with eyes closed, but without a head rest. Previous experiments had led me to fear the effect of such a rest, when working with tones of the present character. My subjects were instructed to eschew all tendency to head movements while making their localizations, and I watched them as closely as possible to detect any such movements. Light, open arm rests enabled them to retain an accurate sense of their general bodily orientation and, after a little practice, readily to assume and retain the correct position. They were trained in the nomenclature employed to designate the various positions on the sphere, and in cases of any doubt they were asked to open the eyes and point. Needless to say, on such occasions precautions were taken to move the semicircle and carriage first, so that their position during the experiment should not be thus discovered. The sounds were given for periods of three to four seconds. This time was hit upon as the result of actual experiments made to determine that duration of the stimulus which would permit clear perception, without any feeling of stress or haste, and at the same time avoid tedium and the confusion sometimes caused by wandering attention. The several tests with different sorts of sounds were made as nearly comparable as possible by using the same

[1] Helmholtz, 'Sensations of Tone,' translated by Ellis, 3d ed., p. 399.

positions. This was, of course, not known to the subjects, who were given no indication of any sort as to the point from which they might expect the next sound. Moreover, the order was altered in which the various kinds of sounds were given at the several positions employed.

Of the three men who served as reagents for me one had had no previous practice in such observations, one had had a moderate amount, and one was extensively drilled. The results gained from all of them agree thoroughly in their fundamental implications, although there is naturally some quantitative variation. I place most confidence in the results of the most experienced observer, and I shall devote myself mainly to his reactions. This is the more warranted by the relatively small number of experiments I have succeeded in making under reliable conditions — some four hundred only. The accompanying table (Table I.) exhibits compactly the results of this observer's localizations:

TABLE I.

(REAGENT, J. B. W.)

Average Error in Degrees.	Sounder.	Reed Pipe.	Bell.	Stopped Pipe.	Tuning Fork.
Longitude,	2	9.5	5	30.5	53
Latitude,	7	4.5	11	13.5	41
Total,	9	14	16	44	94

RESULTS.

We may say at once, that under such conditions as these — *i. e.*, entire freedom from reflection — there is never any confusion of points in one lateral hemisphere with points in the other, save when one approaches very near to the median plane. Even then this form of error is extremely rare and probably attributable to wandering attention, to accidental suggestion from some extraneous source, or to some similarly irrelevant circumstance. The case of pure tones formed no exception to this rule, and the theories which make the intensity of the stimulation of the two ears fundamental in the explanation of localization, are at least correct so far as concerns the assignment of a sound to one of

these hemispheres or the other. Sounds originating in the median vertical plane are also correctly referred to this plane.

When one scrutinizes the table further, however, it becomes clear that within the lateral hemispheres accuracy of localization appears to be a function of the sound. The average error in localizing the tuning-fork is 94°, which is more than a quadrant. With the stopped pipe the error is less than half as large as this, while with the bell and reed pipe it falls to less than a sixth, and with the noise is at its minimum, showing less than a tenth of the error with the fork. I lay no great stress on these precise figures, yet I have no question but that they indicate the intrinsic nature of the differences in the capacities of localizing these different forms of sound. Certainly the objective record was perfectly confirmed by the subjective assurance of the observers and their promptness of localization. Moreover, when, as in certain special experiments, the sounds were repeated two or three times in quick succession with a very brief duration for each stimulus, the accuracy of the localizations with the *complex sounds* was distinctly improved. This procedure seems to have the effect of making the quality differentia more noticeable than when the sounds are more continuous.

The comparison of the errors in latitude and longitude is not entirely free from ambiguity, because no points nearer than 45° to the poles were actually employed for giving the sounds. The observers did not know that this was to be so, but it makes comparison relatively unprofitable. In the case where localization is most accurate, the errors in latitude are notably larger than those in longitude, as one might anticipate from the standpoint of the intensity theory. In the tuning-fork case it would seem that mere chance might in the main be accountable for the results, with a single exception to be mentioned in the next paragraph.

The longitudinal regions immediately opposite the ears show fewer errors and errors of smaller amount in the localization of the pure tones, than do the regions in front and behind this. Indeed, the most striking difference in the localization of complex and simple tones is to be found in the ascription of the

exact location of sounds to the various points in these lunes diagonally in front and behind. This is in accord with Steinhauser's computations upon the effect of intensity.[1] In the vertical plane, in which lies the line joining the ears, the localizations of pure tones are apparently relatively accurate save as regards height. This constitutes the exception above mentioned and seems to agree with Lord Rayleigh's observations and mathematical calculations, showing that the objective differences in the intensity of the sounds reaching the two ears, which is always relatively small under normal conditions, becomes rapidly less as we move away from the line joining the two ears. With the most experienced of my observers the average error of localization in latitude is nearly four times as large as that of longitude in this region.

Taken in their entirety the experiments seem to indicate that even with pure tones, intensity differences alone are sufficient to enable our confident and correct assignment of such sounds (1) to the median vertical plane, (2) to the lateral hemisphere from which they may chance to come, and (3) the further less accurate and less confident determination that certain sounds of this character belong to the vertical transverse plane of the head. But accuracy of localization as regards altitude in this transverse plane and accuracy in the several regions between this plane and the median plane — accuracy such as is commonly possessed, involving an average error of 10° to 25° — is apparently dependent upon tonal complexity and the modifications in timbre which complex sounds undergo through the changes in the intensity of their partials, when heard from different directions. Localization within the vertical median plane is inaccurate with all sounds, but most inaccurate with pure tones.

The matter can be put diagrammatically as in the accompanying cut, which represents the sphere within which the observer sits. (Fig. 3.) Sounds in the planes *FUBD* and *LURD* can, as the intensity theory requires, be localized with considerable accuracy as regards the *plane* to which they

[1] Steinhauser, 'Theory of Binaural Audition,' *Philosophical Magazine* (5), Vol. VII. (1879), pp. 181, 261.

belong. The exact *point* in the plane from which they originate is relatively uncertain, when intensity is the only available criterion. The experiments seem to show with some definiteness that, as we pass from one of these planes to the other, inaccuracy of localization rapidly increases, unless there be definite qualitative differences in the successive sounds. Without such qualitative variations the lune *UEDG* is subject to persistent confusion with the lune *UHDK* and the several points in each lune respectively are subject to gross confusion with other

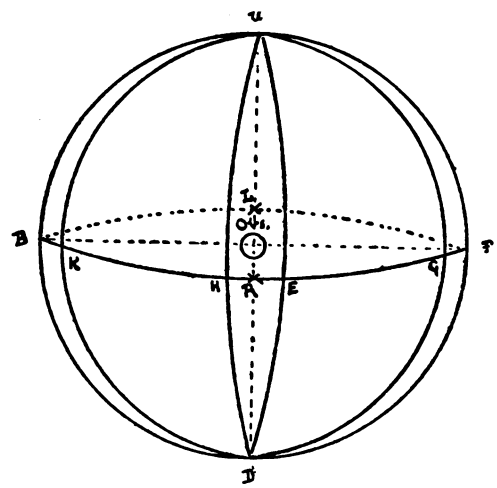


FIG. 3. *F, B, U, D, R* and *L* indicate, respectively, front, back, up, down, right and left.

points in the same lune. Whether the confusion of points in the upper with points in the lower hemisphere is in the case of pure tones notably different in quantity or other characteristics from the confusion of such points with others in the same hemisphere, it is not at present possible to say.

These statements concerning localization as a function of tonal complexity must not be understood as meaning that we are reflectively conscious of this local sign of direction involved in the changing quality, or timbre, of the tones. Sometimes this is noted, but it is not in any way necessary that it should be. Whether or not conscious experience teaches us in childhood to discriminate these varying sensations as having a varying spatial significance, is a question of genetic psychology with which it

is not possible here to deal. Certainly as adults we make the localizations in an almost reflex manner. But the basis of the localizations is found in these symbols reported in consciousness as differences of quality, to which we have come to attach certain space values.

My subjects used much visual imagery in their judgments. My best trained observer seems in his localizations to be conscious of little else beyond such imagery and an occasional tendency to move the head in the direction of the sound. For him to localize a sound means chiefly to get a visual image of the sounding object in the position where he supposes it to be.

The work thus reported opens up the problem which I hope subsequently to work out in more detail. Much fuller observations along the line already pursued are required to permit more confident and inclusive conclusions. Differences in the localization of pure tones of widely varying pitch must be investigated, for the sound shadows involved with such tones and the diffraction experienced by them vary very considerably. It will be necessary to study more carefully the number and character of the partial tones concerned in the differences we have noted. This involves the whole question of relative intensity and pitch in the partials. All one can say at present is that with sounds of medium pitch such accuracy of auditory localization as our common everyday experiences reveal, seems immediately connected with the presence of distinguishable (though not necessarily noticed) partial tones. When such partial tones are absent or very inconspicuous, gross inaccuracy of localization is at once apparent. Detailed information relative to the localization of very high and very low tones is still to be secured. The effect of the duration of the sound upon localizing deserves closer inspection. In connection with several of the points just mentioned the peculiarities of auditory judgments of distance, as distinct from direction, also require more exhaustive investigation than they have as yet received.

In conclusion I wish to express my sincere obligations to Mr. J. B. Watson and Dr. M. L. Ashley, who have given me unsparingly of their time and assistance. I am also indebted to Dr. Warner Fite for assistance in the construction of the apparatus, and to Professor E. W. Mahood for service as reagent.

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

COMMUNICATED BY PROFESSOR JAMES ROWLAND ANGELL.

I.

FATIGUE; SUGGESTIONS FOR A NEW METHOD OF INVESTIGATION.

BY C. R. SQUIRE.

A period of constructive work upon a scientific problem is usually followed by one of criticism. This period of criticism is, however, the precursor of another productive period. For after flaws in the apparatus or in the method of procedure have been brought to light the attempt to remedy them is likely to follow. When the concepts involved are clarified the problem is rendered capable of more definite statement.

We have reached the second period in the study of fatigue. The attempts of Mosso and Griesbach to establish simple and convenient psychophysical methods for the investigation of fatigue, because of their very clearness and conciseness of statement, have been most open to attack. Müller's[1] criticism of the ergograph, trenchant as it is, will not lead us to discard every form of this instrument, until a better one has been substituted. On the other hand the æsthesiometer will scarcely be considered again for an investigation of fatigue by one who is familiar with the literature on the subject.

There is, however, a numerous class of investigations that have met with scarcely any criticism: These may be classed together, since they attempt to measure fatigue in the gross by ability to add or perform some other associative act of greater or less complexity. Most of these studies have emanated directly from Kraepelin's laboratory or have adopted in some particular

[1] 'Ueber Mosso's Ergographen,' *Phil. Stud.*, XVII., 1-29.

the methods there used. Thorndike's[1] criticism was directed against certain dogmas that have arisen as a result of these investigations, rather than against their method of procedure.

This type of investigation appealed to educators. The method was simple; it could, apparently, be directly employed in the schoolroom upon masses of children. This apparent feasibility, together with the lack of wholesome criticism, gave rise to a considerable number of such studies. The total result is a series of pedagogical maxims as to the relative amount of fatigue induced by the various subjects on the school program, the proper alternation of work and rest, and other maxims of similar nature. Unfortunately these maxims rest upon a very insecure scientific basis. The method of procedure is so gross that the results are not susceptible of an unambiguous interpretation.

In a review of an investigation of this class made upon school children, MacDougall[2] makes the following pertinent criticism: "The test does not get at the phenomenon at all directly or unequivocally. The material from which the results are read is the product of the total set of mental conditions obtaining at the time of the investigation; and the number of errors in any given case will as readily be affected by a feeling of rivalry between the pupils or by a momentary distraction as by the influence of fatigue itself. These influences cannot unconditionally be set down as constant factors, which are therefore eliminable. The anticipation of recess or the conclusion of work may very well be potent in establishing a law of rhythmical increase or decrease in the number of errors, which will well combine with the actual exhaustion effects to produce a curve which does not at all truly represent the rise in fatigue."

The most obvious fallacies that have determined the method as well as the interpretation of results in the majority of these investigations are:

1. That muscular and central fatigue are interchangeable terms; that one can be directly expressed in terms of the other.

2. That fatigue arising from one kind of psychical activity

[1] 'Mental Fatigue,' PSY. REV., VII., 466–482; 547–579.

[2] PSY. REV., VI., p. 203.

can be measured by one involving totally different processes. This is one of the most frequent fallacies.

3. That incompetency due to fatigue, and decrease in interest due to the monotony of the task, are one and the same. Few of the investigators discriminate between them.

4. That the number of arithmetical operations, etc., performed during a given time represents the whole of the psychophysical activity, and consequently the percentage of decrease in the number can be regarded as function of increase in fatigue. Leuba [1] pointed this out in his criticism of the Griesbach method.

5. That an experiment can be framed which for an hour or more will be a direct correspondent to the same period spent in the ordinary activities of school.

6. That children are fit subjects for fatigue experiments.

Children are incapable of the prolonged disinterested attention demanded by the fatigue experiment. The only test which can ever be used in investigations upon school children with any hope of obtaining reliable results, must be of a similar nature to Griesbach's. Meumann [2] suggests the use of changes in blood pressure, pulse and respiration as measures of fatigue in such cases. A trustworthy test of this character is highly desirable.

However, it is not the purpose of this paper to criticise earlier investigations; but rather in the light of what has been done to offer suggestions for further study.

What must be demanded of the new method? First, that it make possible the separation of the component elements, fatigue of muscle and central fatigue, and that it select one of these for systematic study. Secondly, that it make use of a definite method of procedure, which shall be carefully regulated, so that all the conditions shall remain constant, or vary in known directions. Thirdly, that the measure and the process measured shall be comparable.

This, as previously stated, is very frequently overlooked in the formulation of fatigue experiments.

[1] 'On the Validity of the Æsthesiometric Method as a Measure of Mental Fatigue,' PSY. REV., VI., pp. 573–598. Cf. also Germann, *ibid.*, p. 599.

[2] 'Entstehung und den Zielen der experimentellen Paedagogik, Deutsche Volksschule,' V.

METHOD.

The method here described was devised to meet these requirements. It seeks to eliminate muscular fatigue in order to observe the rise and general course of fatigue of attention, exempt as far as possible from peripheral distractions. This, of course, necessitates a device that shall be fine enough to register the fluctuations in the attention during the course of the experiment.

A pattern of ten digits written upon a slip of paper was given to the subject, who was required to memorize it before the experiment began. The subject was then asked to tap the pattern given him for a certain length of time. By the use of the devices of tambour, smoked drum and time-marker all variations from the pattern and all changes in the rate of speed could be read at the conclusion of the experiment and paralleled with the introspective report of the subject.

The next consideration was a means of getting at any muscular fatigue that might be induced by the tapping. The ergograph was used for this purpose. The presumption was, that if a certain muscle or group of muscles could do as much work in a given unit of time immediately after the tapping as before it, the muscular fatigue in that particular case was a negligible quantity. This test would be valueless, of course, unless (*a*) the same muscles were used in raising the weight and in tapping, (*b*) the movements made were of the same character and in the same direction in both instances. Both of these requirements were met, as will be seen.

The reagent sat in a swivel chair, raised so that his left forearm rested comfortably in the prone position on the arm-rest of the ergograph. The second and fourth fingers were clamped to the board, and the third finger was enclosed in a padded metal splint for its entire length. By means of the splint all movements of the third finger both in tapping and in raising the weight were restricted to flexion and extension at the metacarpophalangeal joint. The forward and backward movement ordinarily used in raising the weight was converted by means of a system of pulleys devised by Prof. Angell into the up-and-down movement made in tapping. This fact was accidentally

verified. In one of the practice experiments, a disarrangement of the apparatus had delayed the experiment and disturbed the subject. When the experiment was finally begun, the reagent forgot that his finger was attached to the weight and tapped, as he thought, the pattern previously given him. The result was an ergographic tracing of the usual excursion arranged in groups. (A part of this tracing is given below.) A hook attached to the dorsal side of the finger piece that secured the splint and a corresponding S in the end of the wire most remote from the weight made it possible to pass quickly from the ergographic test to the tapping. During the tapping the splint and

finger piece were left exactly as they were when the weight was lifted.

The order of procedure was as follows: The pattern was first given to the subject; when he was confident that he had memorized it, the finger was attached to the weight. At a signal from the operator, the subject, his eyes being closed, began to lift the weight. He continued to do this until he received the signal from the operator to stop. This was given at the end of a minute. No restrictions were made as to the rate at which the weight should be lifted. It was thought that a compulsion to work at any other than the natural rhythm would of itself induce fatigue. The ergographic tracing, together with that of an electric time-marker, was made upon the smoked drum of a kymograph.

After the minimal pause necessary to detach the weight and adjust the lever for tapping, a third signal was given and the subject began to tap the pattern previously learned. The writing point of the time-marker was adjusted as closely as possible under the writing point from the tambour in order to economize drum space. The tapping, except in the practice series, was

continued until the drum was filled or time at disposal of the subject at an end. Some of the later series lasted a little over forty minutes.

The kymograph was then quickly adjusted for the second ergographic test. The lever from the tambour was set aside, weight attached, and the tracing taken for the same time and in similar manner to the first.

The objections that have been raised to the use of the ergograph in fatigue tests were either met by some arrangement of the apparatus or were naturally ruled out through the use we made of it. The leverage was kept constant. This was insured by the arrangement of the splint. Müller attacks the ergograph on the ground that it is impossible to isolate a single muscle or even a group of muscles with it and so study their fatigue. This does not vitiate our results, however, since no matter how many muscles or groups of muscles were active, the same muscles and the same groups of muscles were engaged when tapping as when lifting the weights.

In both the lifting and tapping we were able to discern a movement of muscles of the forearm. This would appear to substantiate Müller's statement.

Another objection that has been made to the ergograph by Müller and others with right is, that the necessary tightness of the band around the third finger hinders circulation and consequently induces fatigue earlier than it would otherwise arise. We were also able to rule this out in nearly every test. The pull of the weight which ordinarily tends to displace the finger piece when the movement is backward and forward, served in this case with the up-and-down movement to hold the finger piece in place. The bands were consequently kept much looser than would have been possible with the usual arrangement.

Moreover, the ergographic test was never continued to the fatigue point. We were not concerned with the curve of muscular fatigue nor with the absolute amount of work done, but with the relative amount before and after the tapping which constituted the fatigue test proper. In the first trial series thirty seconds was taken as a unit for the ergographic test; it was found, however, with the weight of 800 grams which was used through-

out the experiment, that one minute was not long enough, even when followed by the tapping, to induce muscular fatigue. (See table.) It was consequently taken as the unit in all the series that followed.

The tapping in the first trial experiments was continued for twenty minutes, although incipient fatigue was evidenced some time before the end of the twenty minute series; this time, however, was too short to give a fair expression of the course of fatigue. In some of the series the tapping was continued for forty minutes. It was impossible to regulate the time of tapping exactly and get the maximum duration, since we were not always equally successful in economizing drum space. One subject had but an hour at his disposal; when there was a delay in starting the experiment, it was frequently necessary to stop the tapping at the end of thirty minutes. In most cases fatigue of attention became evident after fifteen minutes.

Although all movements tend to become automatic, the experimenter had hoped that a pattern of ten digits would present difficulties enough to prevent its mechanical reproduction within the short space of time given to the experiment. Here a difficulty arose. One subject in particular manifested a very strong tendency to reduce the whole process to automatic action. To obviate this difficulty the subjects were asked to give the series in alternation with the series multiplied by two; on the next day the pattern given was varied in two additional series. The series were made more and more complex until the test consisted of the pattern and four variations. Then the order was reversed, with three, two and one variations, until finally the subjects were simply asked to tap the series memorized. The patterns were as follows:

July 7th	4 4 2 1 3 2 2 4 3 1 then
Series multiplied by two, or	8 8 4 2 6 4 4 8 6 2 then
Ser. multiplied by two minus one, or	7 7 3 1 5 3 3 7 5 1.
July 8th	3 2 2 4 3 2 4 4 2 3 then
Series minus one, or	2 1 1 3 2 1 3 3 1 2 then
Series multiplied by two minus two	4 2 2 6 4 2 6 6 2 4 then
Ser. multiplied by three minus three	6 3 3 9 6 3 9 9 3 6.

July 9th	1 1 4 2 1 3 2 2 1 3
Series plus two	3 3 6 4 3 5 4 4 3 5
Series multiplied by two plus one	3 3 9 5 3 7 5 5 3 7
Series multiplied by two plus three	5 5 11 7 5 9 7 7 5 9
Series multiplied by two minus one	1 1 7 3 1 5 3 3 1 5.
July 10th	3 1 2 1 4 3 4 1 2 1
Series multiplied by three	9 3 6 3 12 9 12 3 6 3
Series multiplied by three plus two	11 5 8 5 14 11 14 5 8 5
Series multiplied by two plus three	9 5 7 5 11 9 11 5 7 5.
July 11th	2 1 3 4 1 1 2 4 2 3
Series multiplied by three minus one	5 2 8 11 2 2 5 11 5 8
Series plus three	5 4 6 7 4 4 5 7 5 6.
July 12st	3 1 1 2 4 3 3 1 3 4
Series multiplied by two	6 2 2 4 8 6 6 2 6 8.
July 14th	2 4 2 1 2 4 1 3 3 2.

The subjects were: Miss H., a principal of a ward school in Chicago; Mr. B., a teacher of mathematics, and Mr. K., also a teacher of mathematics. None of these had previously had much practice in introspection. This lack of previous practice was, however, more than counterbalanced by their interest and the pains which they took to follow all directions given.

After five practice series had been taken, it was evident that the tendency to automatic action in two of the subjects would make it impossible to study the fatigue of attention if the simple pattern alone were reproduced. The course of procedure just described was then begun. The subjects had by this time become familiar with the apparatus and the general course of the experiment.

The hours of experimentation were: *B*, 8–9 a. m.; *H*, 10–11:15 a. m.; *K*, July 1 and 2, 3:30 to 4:30; after that 1:30–3.

In the time at our disposal we could only hope to demonstrate the feasibility of our method.

It now remains to be seen whether the results warrant the assumption that the method above described meets our requirements. Do the results indicate that it was possible to regulate the experiment so that we are able to disregard the element of muscular fatigue? The following table answers this question:

TABLE I.

	H.				*B.*				*K.*			
	Number.		Distance.		Number.		Distance.		Number.		Distance.	
	I.	II.	I.	II.	I.	I.	I.	II.	I.	II.	I.	II.
July 1	46	53	167	206	38	40	147	180	60	61	211	246*
" 14	42	49	147	187	39	42	152	174	54	60	225	220
" 2	50	54	100	108	40	37	137	142	51	68	214	227
" 12	40	47	175	186	35	42	140	164	52	61	246	284
" 7	37	48	116	133	36	36	181	206	63	48*	172	194
" 11	37	47	126	160	36	40	151	183	56	54*	212	270
" 8					34	42	178	228	64	56	280	265
" 10	38	45	156	148*	40	38*	150	164	45	49	196	205
" 9					35	37	133	151	53	53	175	154

This table records the number of times the weight was raised by each subject before (I.) as compared with the number of times after (II.) the fatigue experiment; the distances through which the weight of 800 grams was raised in the two instances are also compared. The results are grouped with reference to the complexity of the series. It can be seen that muscular fatigue was a negligible quantity in most of the series, for with a very few exceptions the weight was raised a *greater* number of times and through a greater distance after the fatigue experiment than before it. All of the subjects at one time or another reported that the excursion they would have made in the second test was cut short by the limitations of the apparatus. This difficulty was never experienced in the test preceding the fatigue experiment. The objective difference in ability to raise the weight in the two instances was thus really less than the subjective, as reported by the subjects. They found relief in the raising of the weight after the strained attention required in tapping. It was undoubtedly this feeling that brought about the constant difference in favor of the second test.

A decrease in the number of times the weight was raised is more frequent than a decrease in the distance through which it was raised. Kraepelin believes that the first factor is conditioned by central fatigue; the second alone by muscular. Our results do not substantiate this theory, however, since there was evidence of central fatigue, when there was a marked increase in the number of times the weight was raised.

The introspective reports of the subjects explained nearly all the exceptional cases.

These cases were found in nearly every instance to be d to some change in the conditions of the experiment, most f quently to a disarrangement of the apparatus. If we disca all those series — marked in the table with an asterisk — i which some disturbance in the regular conditions of the exper ment was noted either by the subject or the observer, our result surely warrant the conclusion that this method gives us a mean of eliminating muscular fatigue; and that the errors made i the production of the series tapped were not due to muscula fatigue. Our results seem to indicate that central fatigue doe not decrease ability to do muscular work.

Thorndike[1] with the dynamometer could find no decided decrease of physical power after continued mental effort.

Bolton[2] found that "Certain forms of mental activity, prob ably such as are associated with *motor* excitations, appear to in crease the height of the ergographic curve, although they un doubtedly produce central fatigue." He says again, "Dennoc beweisen unsere Versuche mit vollster Bestimmtheit dass e nicht angeht, aus dem Sinken der Muskelkraft irgend welch Schlüsse auf den Grad der geistigen Ermüdung zu ziehen, un so weniger als Oseretzkowsky auch nach dem sehr ermüdende Auswendiglernen eine erhebliche Zunahme der Muskelleistun fand. Wahrscheinlich sind die gegenseitigen Beziehungen seh verwickelte. Bevor also Massenversuche angestellt werden, is es unumgänglich nothwendig, mit grösster Sorgfalt der Wech selwirkung zwischen geistiger Thätigkeit und Muskelleistung in einzelnen nachzugehen, damit wir erst ein richtiges Verständ niss dafür gewinnen, was wir eigentlich messen."

We quote Bolton at length as he here emphasizes the neces sity of a method of procedure similar to that followed in thi experiment, *i. e.*, the elimination of one factor in order that w may more accurately measure the other.

It is, however, surprising that one is capable of such pro longed and fairly rapid contraction of the muscles of the finge as is necessitated in the tapping of these patterns for forty min utes, without inducing well-marked muscular fatigue.

[1] 'Mental Fatigue,' PSY. REV., VII., 578.

[2] 'Ueber die Beziehungen zwischen Ermüdung, Raumsinn der Haut un Muskelleistung,' *Psy. Arb.*, IV., 187, 232.

Take one record, that of *B* on July 14, for illustration. We find that the pattern was repeated one hundred and twenty-three times before the close of the series. The pattern when completed requires twenty taps. If we neglect those taps that were repeated and omitted — the one will probably nearly balance the other — the total number of taps made by *B* in that record was 2,460. This is by no means the greatest number of taps made during any one series, for the complex patterns required a greater number of taps for the complete pattern and were frequently repeated quite as many times as the one taken for illustration. When we turn to the table, it is seen that there is no evidence of muscular fatigue in the ergographic record taken after this tapping.

Moore[1] says that he once made 480 taps, although he was sure that he could not have made one hundred more at the close of any record. In his records fatigue was manifested as early as the seventieth tap. The early rise of fatigue in his case may have been partly due to the arrangement of the apparatus; the tapping may have been violent and at a speed exceeding the natural rate of the subject. (While it is not the aim of this paper to discuss the question of the rise of muscular fatigue, the query suggests itself whether tapping could not be prolonged indefinitely, if circulation were unimpeded and the natural rate of the subject maintained, without giving rise to muscular fatigue.)

The record of *H* for July 2 is free from any trace of fatigue. Her rate was very slow; she followed the rhythm of the time-marker, one tap a second. At this rate she was able to give series after series without making an error. The rate here is much slower than that of the other subjects and also than that of her own subsequent records. In her introspective report she notes that this series was not so fatiguing as that of the day previous; because of its variety it was more interesting and therefore easier to attend to.

Is not the early rise of fatigue in Moore's experiment due in part to the direction of the attention upon the contraction of the finger and the sensations involved? In our problem the atten-

[1] 'Studies of Fatigue,' *Studies from Yale Laboratory*, III., 92.

tion was necessarily directed upon the pattern and the associations called for. Is it possible that the central excitation retarded the rise of muscular fatigue? However that may be, muscular fatigue did not play a noticeable part in these experiments and can be disregarded when we attempt to explain the irregular and inaccurate tapping.

It will surely be apparent at once that our second requisite of a satisfactory method was fully met; viz., that it was possible to regulate the method of procedure so that the conditions should remain constant or vary in known directions.

It can also be seen from the description of the method that the measure and the processes measured are comparable, since by the terms of the experiment the one is contained in the other. The measure of fatigue was the increase in the number and duration of errors and irregularities during the production of the memorized and associated series.

The errors and irregularities were of various sorts; taps added or omitted, groups added or omitted, groups broken by long pauses, whole groups or series repeated, irregular succession of groups in which there was no trace of the pattern given and at times the whole series of ten digits was omitted.

None of these inaccuracies or irregularities as such can be considered indices of fatigue; lack of practice would also cause irregularities; but irregularities due to this cause would *decrease* as the series were repeated. Irregular following upon a long period of fairly correct tapping, *i. e.*, after practice has had its maximum effect, can hardly be regarded otherwise than as a pure fatigue phenomenon, unless it be due to lack of interest and consequent inattention. Lack of interest could not be advanced as an explanation in the case of these subjects.

As fatigue enters, recall is halting and often fails, the associations weaken. As the associations fail, some appear to hold together better than others.

K in a certain series had firmly associated the combinations 5, 8, 5 and 3, 6, 3. As he became more and more fatigued, he interspersed these with groups from all the series regardless of order of groups or series. Again, the first five or six groups of the first series were more closely associated than the last mem-

bers of this series; so were the last five or six groups of the second series, than the first members of that series. As the experiment was continued and fatigue became apparent, he frequently omitted the intervening groups, although he never failed to give these in their correct order. This is doubtless explained by the fact that all of the subjects tended to divide the series of ten digits into smaller series. Thus the groups memorized together were undoubtedly recalled together.

The results show that:

1. Gross irregularities become more frequent as the series are continued.

2. The increase in inaccuracy and irregularity is not continuously progressive, but shows fluctuations.

3. While the complex series, as we should naturally expect, show a greater number of errors at the beginning of the record than the simple series, the proportion of increase as the record proceeds is really less than in the case of the simple series.

Certain of the complex series have not only a relative advantage over the simple with regard to the increase in inaccuracy, but an absolute. The number of errors was least for *B* when one variation of the original series was introduced; for *H* and *K*, when the original series had two variations added.

Scripture[1] found that "The fatigue in reaction time increased with the complexity of the adjustments required for perceiving the stimulus. There is least fatigue where only an effort of attention is involved, more when the act of accommodation is added and still more when the act of convergence is added."

The two instances, though they might appear comparable at first sight, are not. An experiment in reaction time by the very conditions of the experiment keeps the attention on the alert; each stimulus gives a new impulse. With the tapping the control was central; no external stimulus served to reinforce the central mandate. Therefore, the greater the monotony, the greater the strain on the attention and the more fatiguing the series.

H says: "At times the deadly monotony of the tapping

[1] 'Researches in Reaction Time,' *Yale Studies*, IV., 12.

nearly overcomes me." At such times peripheral stimuli tend to force themselves upon the attention and lapses occur.

The most complex series were also extremely fatiguing; the associations were not so readily made; there was also a greater number of things to be memorized. When the pattern was *slightly* varied, the demands upon the attention were not so great as with the simple or very complex series.

The effectiveness of the simple as compared with the complex series in producing fatigue, varied with the individuals. *H* always found the simple series very fatiguing because of their monotony; while *B* says: "The simple series produce less fatigue. They become in part automatic."

With the most complex series the subjects frequently reported a feeling of relief as they returned to the original pattern. One variation of the original series was at times apparently easier to give than another just preceding it or following it. This might very well be expected, since more associations were required in some cases than in others, etc. To double a series would naturally be less difficult than to double and then to subtract one.

A general periodicity in the number of errors was to be observed. A similar phenomenon was frequently noted in the height of the excursion of the writing point. Such records began with a maximal excursion, which seldom lasted beyond the second rotation of the drum, a period of about five minutes' duration. After this the excursion grew smaller quite rapidly. Then followed frequent fluctuations in the height of the excursion, although the maximal height was never reached again.

The more complex the series, the less marked was the rhythm above noted. In these series the excursion throughout the record was slight and quite even. This phenomenon was not frequent enough to make a detailed study of it possible.

However, it is possible by means of the irregularities in the tapping to follow the general course of the fluctuations in the attention.

The facts brought out by the records are:

1. The duration of the fluctuations increases as the series progress.

2. Their frequency increases also, although not continuously.

3. Their duration and frequency are less in those complex series which give the advantage of variety without calling for too many and too involved associations.

A period of prolonged inattention or inaccurate and irregular tapping is always followed by one fairly free from fluctuations. Kraepelin[1] and Voss[2] have noted the same phenomenon and have given it the name of '*Müdigkeitsantrieb*.' This is especially noticeable in *K's* record.

It was impossible to rule out all distractions arising from external stimuli. Those most frequently noted were:

1. Click of the time-marker, strong tendency to follow its rhythm especially in the early series. "After the first tap there was a tendency to wait for the time-marker to click before making the next tap. This gave an opportunity for the attention to wander, especially toward the sound of the marker, and often resulted disastrously."

2. The sensations resulting from the impeded circulation consequent upon the binding of arm and finger.

3. Constrained position of the body. *H* resented the fact that she was obliged to close her eyes. This resulted in a distraction of the attention.

4. The pounding of the workmen outside, mentioned only by *H*.

5. The click of the metal finger piece, which was heard when the splint became disarranged, served at times as a distracter, at other times as a stimulator.

The fluctuations, however, were not dependent upon external causes. Neither were they subject to voluntary control, as the following report shows: "Could anticipate lapses of attention but not ward them off. The feeling was not similar to that experienced in muscular fatigue. Merely a sense of the inevitableness of the impending lapse. When the lapse came, it was not recognized as such until after the mistake had been made. This, of course, called forth effort to prevent further

[1] 'Ueber Ermüdung und Erholung,' *Psy. Arb.*, I., 678.

[2] 'Ueber die Schwankungen der geistigen Arbeitsleistung,' *Psy. Arb.*, II., 409.

blunders, which were generally prevented, but a sense of strain was always felt before the work would run smoothly again. Moments of especial clearness were observed when I could remember the series for quite a distance ahead. These may be the crests of the attention wave. They were not subject to control."

These are not the normal fluctuations of attention, which are present even in the most intense attentive state. Several important differences in the normal and the fatigued attention wave are to be noted. (1) The normal fluctuations are much shorter in duration. (2) The normal are more regular in their order of occurrence. It is only necessary to observe the records near the close of a long and difficult series to note the great irregularity of the fluctuations in a state of fatigue. (3) The lapses are more intense and recovery more difficult in the fatigued state. All of the subjects report occasions of total blankness. *K* experienced them most frequently as times when he is 'entirely at sea as to which of the series was being tapped.' *B* says: "Sometimes I have felt completely lost, utterly in the dark for an instant. Recovery in this case, although rapid, was with unusual effort." (4) When the subject is fatigued, voluntary control is weaker; he is not able to shut out distractions that would not trouble him ordinarily. The simple series gave more occasion for fluctuations of this nature than did the complex. The ease of the series and the monotony of the task favor mind-wandering in the most conscientious subject. *K* compares this state to that experienced when fatigued by adding long columns of figures. With the complex series, peripheral distractions are not so frequent a cause of fluctuations. In these series, lapses are due to difficulty in making proper associations and to halting recall of groups and of series. "The interval which was required to decide what series should come next was often long."

What is the cause of these fluctuations? It is most probable that they have much in common with fluctuations of attention under normal conditions. In case of fatigue, however, innervation is accomplished with greater difficulty and is therefore accompanied by sensations of bodily strain or a 'feeling of

effort' as reported by the subjects. Thus the period of uncertainty is lengthened and an external stimulus such as the click of the apparatus may serve either to enforce or distract, according to the disposition of the individual.

A decrease in rate of speed has been regarded as indicative of fatigue; some experimenters have used it alone as a measure of fatigue.

Gilbert[1] in his experiments upon school children found that the loss of speed in tapping for 45 sec. at six years was 21.4 per cent.; at fifteen years 12.7 per cent.

Moore[2] found that fatigue lengthened the time, although the increase was not a steady one but showed fluctuations. Oehrn[3] repeats the statement frequently made by Kraepelin. "Two factors influence the duration of psychical processes. Practice produces an increase in their rate; fatigue a decrease." Scripture[4] found in his experiment on reaction time that a lengthening of the average times appeared sooner than an increase in their mean variations.

We were at first inclined to accept the current opinion, but our results, although most certainly indicative of fatigue, do not show any regular decrease in the rate of tapping.

The series were divided into five-minute sections, and the average duration of each of the patterns during the successive five-minute periods was found. Often there was an increase in the rate of speed as the tapping continued, although there was well-marked fatigue. On the other hand, while the size of the M. V. decreased for a time with practice, it gradually increased toward the close of the series.

It is quite as likely that decrease in rate of speed is due to a feeling of weariness, of tediousness. As that is an index of fatigue, the first is often confused with the latter. While this decrease of rate may be expressive of fatigue, it is not always and necessarily so. A certain irregular and fitful tapping at increased rate is quite as much an index as decreased speed.

[1] 'Mental and Physical Development of School Children,' *Yale Studies*, II., p. 40.

[2] *Op. cit.*, p. 95.

[3] *Psy. Arb.*, I., p. 92.

[4] *Op. cit.*, p. 19.

Some of the testimony clearly cited is in perfect harmony with this presumption. Gilbert found the decrease in speed greatest in the case of the young children who are less able to resist a feeling of tediousness, and less capable of a prolonged voluntary effort, than are adults. Our subjects were unusually free from such an influence; interest in the problem kept them at a point of the highest possible efficiency.

In our opinion the size of the M. V. is a much more accurate measure of fatigue than the average rate of speed. It can be regarded as the coefficient of irregularity, and together with the purely qualitative measure — the degree of accuracy — gives us a fairly reliable standard, when the method of experimentation does not take into account the duration and frequency of the fluctuations of attention, which after all furnishes the most accurate measure.

In our previous discussion we have taken it for granted that there is a state of fatigue, a condition of real incompetency. The fatigue induced by these experiments was not always general, or marked by incompetency in *all* activities of the day, during the hours that succeeded the experiment.

While the possibility of a state of general fatigue is conceded by nearly all investigators, there does not seem to be any recognition of the fact that real fatigue may exist without being general; that there is a state of special fatigue.

Kraepelin[1] distinguishes between '*Müdigkeit*' and '*Ermüdung*.' MacDougall[2] distinguishes between 'weariness' — personal and fluctuating and dependent upon a superficial act of attention — and 'fatigue' — a deep-seated nervous phenomenon. These classifications are helpful in calling attention to the fact that all which passes under the name of fatigue is not genuine fatigue, but is often a feeling of boredom with a distasteful task. However, if any piece of work is continued long enough, there arises a condition which we may well call special fatigue or real inability to continue the special activity at anything like the highest possible efficiency. It is a state of nervous exhaustion, though not so wide spread as when fatigue is general. Possibly a totally different activity could be engaged in directly

[1] *Op. cit.*, p. 623.
[2] *Op. cit.*, p. 203.

after the subject had experienced special fatigue without a noticeable failure in either quality or quantity of work.

Thorndike errs in failing to recognize special fatigue. We cannot measure ability for one kind of work by a test which involves other mental processes. It is not surprising, when we consider this fact, that Thorndike found school children were able to do as good work after school as before. "Why I do less work of some special sort after being engaged in it for a long time is not that I am not able but because I don't feel like it," is only partially true. If the work were strenuously continued for any considerable length of time, there would be real inability to do that *special* work effectively; although there might not be a decrease in the ability to do other work, which would be the case if the fatigue were general.

This method enables us to distinguish between general and special fatigue. It should also be possible by gradually lengthening the series to ascertain for each of the individuals the point at which special fatigue became general.

In these experiments we have been chiefly concerned to find a satisfactory method for the study of the fatigue of attention. The method could be used, however, to investigate the effect of fatigue upon recall. As we used it, it is not adapted to the investigation of the effect of fatigue upon the accuracy and readiness with which associations can be made. We found that the variations of the original pattern after a few repetitions were memorized as series; the associations or arithmetical operations called for by the terms of the experiment were omitted and the reproduction of the series became simply a matter of recall. However, the relation of fatigue to association can be investigated, as will be seen, by a slight alteration in the original method.

By a modification of the experiment, it would be possible to determine the effect of fatigue upon the perception and the time interval between stimulus and perception. Muscular fatigue might be eliminated as it has been in this experiment. In place of a memorized series the subject could be required to repeat the stimulus given by the experimenter, or still better by some apparatus arranged to give any desired number of stimuli in any desired order of succession. The fatigue experiment in this

case would be one in simple reaction time carried to the fatigue point and beyond.

This modification, with another slight variation in the method of procedure, would enable us to study the effect of fatigue upon the ease and accuracy with which associations are made. It would simply be necessary to require the reagent to make some definite associative reaction instead of giving a repetition of the stimulus. The ergograph furnishes us with a fairly reliable means of eliminating muscular fatigue in any of these cases.

II.

Meaning in Memory and in Attention.

BY KATE GORDON.

I. Memory as Dependent upon the Complexity of its Content.

It would require no small degree of skill and patience to give an adequate summary of all that has been well done and well said upon the subject of memory and its laws. Without attempting that, we may take it for granted that one or two tendencies are sufficiently apparent. In classifying the laws of memory it has generally been held, by recent writers at least, that there are, in the last analysis, two mutually irreducible principles of association. One principle stands for a connection between things whose first conjunction was fortuitous and whose bond is purely mechanical; the second represents a connection between objects which seem peculiarly fit to go together or which have some intrinsic relation quite apart from simple contiguity. In Wundt,[1] for example, we find the following: "Fasst man so * * * alle Associationen als Resultanten elementarer Verbindungsprocesse zwischen einfachen Empfindungen oder relativ beschränkten Empfindungscomplexen auf, so sind nun an und für sich nur zwei solche Elementarprocesse möglich," * * * *i. e.*,* * * die Verbindung gleicher Elemente, und die Verbindung solcher, die durch gemeinsames Vorkommen in einen

[1] Grundzüge d. Phys. Psy., vol. 2, p. 468.

functionellen Zusammenhang getreten sind. Wir wollen diese beiden Formen der Elementarverbindung die Gleichheitsverbindung und die Berührungsverbindung nennen." These two forms are elsewhere called intensive and extensive, or internal and external association. Aschaffenburg [1] quotes in his work a number of proposed classifications which adhere to this same twofold division, for example, that of Trautscholdt as follows:

I. *External Association* (*i. e.*, by contiguity).

A. Of simultaneous ideas.

i.—2.—etc. (subordinate divisions).

B. Of successive ideas.

i.—2.—etc. (subordinate divisions).

II. *Internal Association* (by similarity).

In the other schemes of division which Aschaffenburg quotes (Kraepelin, Wahle, *et al.*) and in his own, the distinction of inner and outer association is maintained apparently as ultimate. The reaction against the extreme form of associationism as held by Mill, Bain and others is represented in Stout [2] by the great importance which he attaches to the *conative unity* of consciousness. Stout also seems to uphold the classification of associations as external and internal. He calls them respectively the 'general unity and continuity of consciousness' and the 'conative unity and continuity of consciousness,' meaning, apparently, accidental and intrinsic connections between psychic elements. In Mr. James's categories of frequency, recency, vividness and emotional congruity,[3] we recognize, in the frequency and recency, the external or mechanical conditions of remembrance, and in the vividness and congruity, the intrinsic conditions which stand for the amount of attention and interest bestowed. James's analysis of the 'similarity' association, however, tends to reduce the usual sharpness of the difference between contiguity and similarity, inasmuch as he points out that similarity is a case of partial identity, which brings it back to terms of the contiguity of elements.

[1] 'Experimentelle Studien u. Associationen,' *Psych. Arbeiten*, Vol. I., p. 205.

[2] 'Manual of Psy.,' pp. 71 ff.

[3] 'Psychology,' Vol. I., p. 577.

If we accept the division of associations into external and internal, we may say that the greater part of the experimental work in memory has been engaged with the external or mechanical factor. The experiments of Wundt and Ebbinghaus, for instance, deal mainly with the influence of repetition and of time-intervals upon memory. The question in this type of experiment seems to be this: given a certain content, by what device can it best be impressed? In the experiments reported below the object was to study the nature of the content itself as affecting association, to find out what sort of material was impressive in itself. If we may say that the essential fact about memory is that earlier conscious process functions by representation in later conscious process, then we may call the test of effectiveness or goodness of memory the likelihood of recall which a given content enjoys. The question here at issue is: what effect, if any, does the value of a psychical object have upon its chances of reproduction? The experiments were devised under the assumption that meaning or value is to be defined on the functional side in terms of attention and interest, and on the structural side in terms of complexity; that the more complex a content is, the greater the numerical aggregation of elements, *i. e.*, the greater the number of its differentiæ, the more it means.

Among the writers who have recognized the quality of the object as a factor in association are the following. Külpe[1] says, in his chapter entitled 'Incentives to Reproduction and Liability of Reproduction,' that the liability of reproduction depends upon the nature of the conjunction of sensations in consciousness. Besides spatial and temporal colligation, he there mentions as important (*a*) the remoteness of impressions from each other in space and time. "The greatest liability of reproduction is correlated with a direct contiguity or succession." (*b*) The more a content differs from its surroundings, the easier will be unification and recall. (*c*) The existence of a name for the complex makes it more effective for central excitation.[2]

[1] Külpe, 'Outlines of Psy.,' pp. 199–202.

[2] *Cf.* also Lehmann, *Phil. Studien*, Vol. V., p. 135 and F. Angell, *Phil. Stud.*, Vol. XIX., p. 1.

He says, further, that the more individual the quality of the connected content, the stronger is the liability of reproduction. "The simple sensation, which * * * can occur in the most various connections, does not as a rule possess any considerable effectiveness for central excitation. But more complicated processes, ideas, are often quite individual in character, etc." Kennedy[1] writes: "The accuracy of recollection is largely conditioned by the character of the content to be remembered." There are [he is summarizing the doctrine of others] two general ways in which a thing may be remembered. (1) By immediate recall, as when fine shades of color are reproduced just as they were in reality. (2) By mediate recall, in which some third factor enters. This is the way in which complex material is remembered; it is fixed by our concepts or classifications of it (*i. e.*, by translations or indirect means). Attention, repetition, rhythm and the general character of the object—these, he holds, govern the 'reception of the object.' "In general we may say that the reception which an object gets into consciousness depends very largely upon the sort of object it is." The idea expressed by the two men just quoted is, of course, commonly remarked, but experimental evidence has been rather more meager.

A research was conducted by Binet and Henri[2] upon 'La mémoire des mots' and 'La mémoire des phrases.' The authors here distinguished the memory of sensations from the memory of ideas. They used as content to be recalled, first, lists of figures (memory of sensations), then lists of isolated words, and finally words in rational combination, *i. e.*, sentences (memory of ideas). They found that words were recollected more easily than figures, and that among such isolated words those were most likely to be recalled which were most intimately connected with the subject's own habits. Sentences were better remembered than an equal number of disconnected words. It should be remarked that although Binet and Henri were working in this last research upon the memory of *ideas*, they appeared to make the exact verbal reproduction the test of correct

[1] 'On the Experimental Investigation of Memory,' PSY. REV., Vol. 5, 483, 488.

[2] *L'Année Psy.*, Vol. 1, 1–23, 24 and 25.

memorizing; substituted words or equivalent expressions were not accepted as being equally good as the precise phraseology of the original material. Miss Calkins[1] and Mr. Kirkpatrick[2] have severally made experiments comparing the memorizing of the abstract (*e. g.*, of words) with that of the concrete (as of pictures). They reached the same general conclusion, namely, that 'the superiority of concrete (objects) to verbal, as memory material, is under-estimated rather than over-estimated even by its most strenuous upholders.' Particularly was this true in the case of delayed as distinguished from immediate recall. W. G. Smith[3] has studied the relation of attention to memory. Smith's experiments illustrate the effects of the distraction of attention from the memory process. Various forms of distraction were used which proved progressively engrossing, with the result that memorizing became poor in proportion as attention was withdrawn. Smith's general contention is that contiguity is a merely formal factor and that the real explanation of effective memory lies in attention and interest. Bigham and Münsterberg[4] have made investigations (with which those of Cohn[5] also agree) showing that the memory of words is more certain if they may be both seen and heard by the subject — that the apprehension by several avenues is better than by one. Miss Calkins in another investigation[6] says that the readily associated objects are the 'interesting' ones; and interest she analyzes (in agreement with James) into the primacy, the frequency, the recency and the vividness of the content presented. The outcome of her work shows that the material which does sustain these relations is that which is best remembered. Margaret Keiver Smith[7] in a study of 'Rhythmus und Arbeit' has shown the effect of variety in rhythm upon association. Nonsense-syllables were learned by heart, the number of necessary repetitions being the test of readiness of memory. It was found

[1] Calkins, PSY. REV., Vol. 5, 451.
[2] Kirkpatrick, PSY. REV., Vol. 1, 602.
[3] 'The Relation of Attention to Memory,' *Mind*, N. S., Vol. 4, 47.
[4] PSY. REV., Vol. 1, 34.
[5] *Zeits. f. Psy. u. Phys. d. Sinnesorgan.*, Vol. 15, p. 161 ff.
[6] PSY. REV., Vol. 3, p. 32.
[7] *Phil. Studien*, Vol. 16, p. 197 ff.

that, other things equal, the syllables which were presented in simple rhythms were harder to acquire than those which came in the more complex rhythms. Thus, syllables were learned more readily in the dactylic than in the iambic meter. Allin[1]

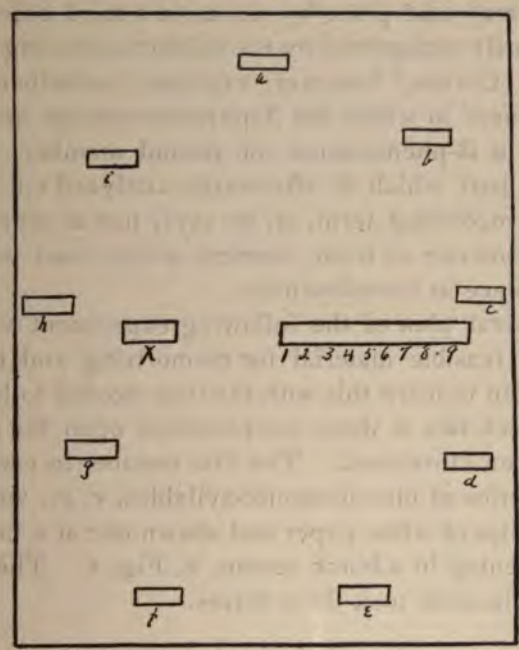

FIG. 1.

S represents a black screen 22 by 28 inches in which various small openings were cut and behind which a revolving wheel carried the printed syllables.

Case I. The syllables appeared one after another at *x*.

Case II. Syllables were shown one after another along the line 1-2-3-4.

Case III. They appeared along the same slot but in a haphazard order, *e. g.*, 5-3-1-9-6-8-7-4-2. This order would remain constant during the learning of any one series of syllables, but was changed every time a new series was begun.

Case IV. Syllables were shown in regular succession around the circle *a-b-c-d*.

Case V. They appeared in irregular succession, as *e. g.*, *f-g-c-e-h-i-a-b-d*.

is also an advocate of the general standpoint which grounds effectiveness in association upon attention and interest.

It may be in place to mention here one or two opinions on the nature of the memorizing of a complex content. Kennedy, as above quoted, distinguishes mediate and immediate recollec-

[1] 'Grundprincip. d. Association.'

tion, and says that the memory of complexes is mediated by some third factor intervening between the two primary members of an association. Scripture[1] and others allow the legitimacy of such a description and have shown that association between two objects may take place by means of a third object which is not consciously recognized by the subject as having performed that office. Cordes,[2] however, explains 'mittelbare Associationen' as cases in which the A-phenomenon (or first member) calls up as a B-phenomenon (or second member) a complex idea. The part which is afterwards analyzed out and called the third or mediating term, is, he says, just as much a part of the B-phenomenon as is the element which stood in the more prominent place in consciousness.

The general plan of the following experiment was to select the simplest feasible material for memorizing and to compare the time taken to learn this with the time needed to learn a content into which two or three complications upon the simple first form had been introduced. The first member in each comparison was a series of nine nonsense-syllables, *e. g.*, vax, tir, etc., printed on slips of white paper and shown one at a time through the same opening in a black screen, x, Fig. 1. The complication of this situation took three forms.

1.

In the first form of the experiment the content was complicated by adding to it a spatial differentiation. The list of nonsense-syllables, instead of appearing in the same place, x, appeared not only in temporal succession but also in spatial succession. The nature of this displacement was varied so that there were in all five different cases to be compared.

Each series was repeatedly shown to the subject until it was learned by heart. Thus the number of repetitions stood as the test of the difficulty of a series. After an interval of three days the same five series were learned over again in the same way, thus giving a test of their effectiveness in delayed recall. In order to distribute evenly the effects of fatigue and of practice

[1] 'The New Psychology,' pp. 201–202.

[2] Cordes, 'Ex. Untersuchungen,' *Phil. Stud.*, Vol. 17, pp. 73–77.

during a sitting, the five sorts of series were presented from week to week in revolving order: *e. g.*, (1) I., II., III., IV., V. (2) II., III., IV., V., I., etc. In the subjoined tables the Roman numbers stand for the five kinds of space relation, the Arabic numbers represent the repetition necessary for correct reproduction. Each of these numbers is the average from five tests.

FIRST LEARNING.

I.	A.		B.		C.		D.	
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.
I.	4.75	.75	5.4	.72	4.2	1.04	7.0	2.00
II.	4.25	.75	4.6	.88	4.6	.48	8.4	2.08
III.	4.50	1.00	5.2	.64	5.4	.88	7.8	2.56
IV.	4.75	.375	4.0	.40	4.2	.84	8.4	.72
V.	6.50	1.50	4.8	.64	4.2	.64	8.0	1.60

SECOND LEARNING—THREE DAYS AFTER.

I.	3.00	.50	2.8	.64	3.0	.40	3.6	.72
II.	3.75	.375	3.2	.64	3.0	.40	4.2	1.44
III.	3.50	1.00	3.6	.48	3.6	.48	4.4	1.68
IV.	2.50	.50	3.0	.40	3.0	.40	4.4	1.16
V.	3.75	.75	4.0	.80	3.2	.96	6.0	1.60

	E.		F.		G.		H.		I.	
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.
I.	5.2	1.04	6.2	.84	15.0	2.80	11.5	.87	5.2	.64
II.	4.4	.64	5.6	.72	11.00	2.0	9.0	3.50	4.6	1.52
III.	4.8	.96	8.0	2.00	16.00	5.2	10.5	2.00	4.2	1.04
IV.	4.0	.80	6.0	1.20	18.40	4.88	8.0	.50	4.0	1.20
V.	7.2	1.84	8.4	1.68	17.6	2.48	11.0	2.00	4.0	.80
I.	3.8	1.04	4.8	.64	9.2	2.56	4.75	.37	3.0	.40
II.	3.0	.40	4.0	.80	7.0	2.80	3.75	1.12	2.6	.48
III.	3.6	.72	4.4	.88	6.4	.84	5.00	1.00	3.0	.40
IV.	3.0	.40	4.4	.88	7.4	2.08	3.75	.75	2.6	.48
V.	4.2	.64	5.4	.88	7.8	1.04	5.75	1.75	3.4	.72

	B′.		E′.		F′.		General Average of all the Subjects.
I.	3.8	.64	5.6	1.52	5.4	.72	7.05
II.	3.8	.96	3.8	.64	4.6	.48	6.14
III.	4.0	.80	4.0	.40	5.2	.32	7.11
IV.	3.2	.64	3.2	.32	4.0	0	6.60
V.	4.2	.64	5.8	1.04	5.6	1.28	7.70
I.	2.6	.48	2.6	.72	3.4	.48	4.06
II.	2.8	.44	3.6	.48	3.2	.32	3.80
III.	3.0	.40	2.8	.64	4.2	.72	4.08
IV.	2.6	.48	2.2	.32	2.8	.32	3.63
V.	2.8	.44	2.8	.64	4.0	.80	4.61

The capitals A, B, C, etc., stand for the different subjects. The right hand column of figures under each subject shows his average variation.

The tables lettered B′, E′, F′ represent a second series of experiments performed with B, E, and F, respectively. They show that the same tendencies which were apparent after five weeks' practice also persist at the end of ten weeks. The above data are not, of course, extended enough to warrant very detailed conclusions, but they point on the whole toward the uniform preference for the two forms which offer both complexity and regularity. The fourth form, which gives the best combination of individuality with unity, was easiest for nearly every person. The introspective evidence of the subjects agreed very closely with their results. Without any suggestion from the operator the following points were volunteered: that the fourth form was easier because each syllable had a place of its own: that it was pleasanter to anticipate a series in that arrangement than to keep the eyes fixed on one point as in the first form.

2.

The second part of the experiment was arranged to compare (I.) the learning of syllables printed in black letters upon white paper with (II.) the learning of syllables printed in black but each upon a slip of different-colored paper. Thus a series

FIRST LEARNING.

	J.		K.		L.		M.		N.		General Average.
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	
I.	5.9	2.30	6.75	1.25	5.4	1.28	8.33	1.77	3.83	.83	6.04
II.	5.3	1.22	6.25	.87	4.4	1.52	8.33	1.33	4.16	.83	5.62

SECOND LEARNING—THREE DAYS LATER.

I.	2.7	.56	4.75	.75	2.6	.48	4.33	1.00	2.66	.50	3.40
II.	2.3	.62	4.00	.50	2.0	.0	3.16	.80	2.50	.50	2.79

would consist of *vax* on a blue slip, *sed* on yellow, *ver* on rose, etc. No color was ever associated with more than one syllable. The subject was not required to remember or even to notice the colors — they were simply there to help or hinder as they could.

The syllables in both these cases, I. and II., were shown in temporal succession but without spatial displacement.

Thus four out of five did their best work with the colored backgrounds. Here too the subjective evidence agrees. In several instances the color was definitely ascribed as the cue by which the syllable was recalled.

3.

In the third form of the experiment, the simple series I. was contrasted with a series II., which combined both space and color differences. The first set was shown at *x*, Fig. 2, and the second set down the line 1-2-3-4.

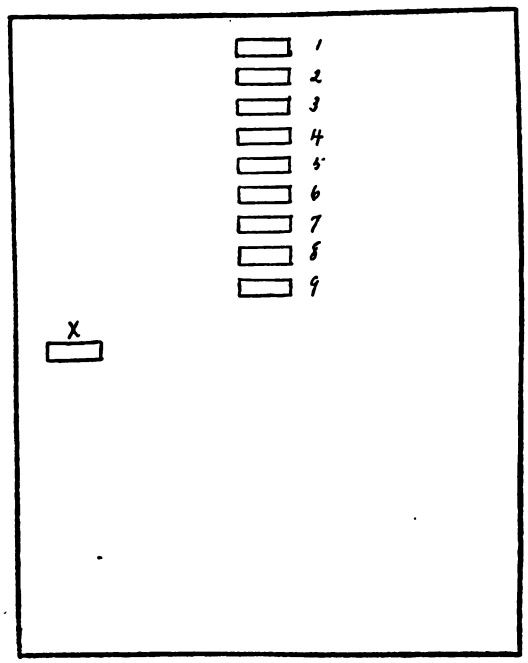

FIG. 2.

The results were as follows:

FIRST LEARNING.

I.	O.		P.		Q.		R.		S.	
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.
I.	15.0	2.8	3.6	.48	3.16	.55	7.0	2.00	3.83	.55
II.	12.4	2.48	3.2	.32	4.16	.28	6.2	1.44	3.83	.55

SECOND LEARNING.

I.	11.0	1.2	2.6	.88	2.33	.55	4.6	.88	1.66	.53
II.	9.2	4.6	1.6	.48	2.33	.42	4.2	.64	1.16	.33

	T.		U.		V.		W.		X.		General Average.
	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	av.	av. var.	
I.	4.33	.66	5.2	.58	2.66	.66	5.5	.75	7.0	2.0	5.728
II.	4.50	1.00	5.2	.84	3.00	.33	4.2	.62	6.0	1.5	5.274
I.	2.83	.55	2.83	.94	1.83	.27	3.25	.37	4.25	.87	3.709
II.	2.66	.44	3.83	.66	1.50	.50	3.00	0	3.25	.75	3.264

The results of all three forms of the experiment indicate that the complex series has the advantage in each case. Not only is this true for the first acquisition, but it holds as well for the second learning. We should have, therefore, to modify somewhat an opinion which is often expressed, namely, that the more difficult a thing is to acquire in the first place the more readily it is regained at a later time. There must, of course, be an early limit to the arbitrary complications which can be introduced in the above way; for as soon as we take a content which cannot be immediately apprehended as a whole, we shall have a division of attention. 'Greater complexity' in a content does not mean a greater number of different objects in juxtaposition; it means greater variety in unity. A complex content is one which, while it represents many possibilities of distraction, *i. e.*, stands for much past experience, may yet be immediately taken in as a whole. Each member of a series, then, must have unity as well as variety, and when we reflect that the combinations used in the tests — a syllable, a color and a point in space — were purely arbitrary and perfectly new to the subjects, it seems somewhat remarkable that there should have been any uniformity at all in the results. The general conclusion to be derived from the three experiments is this: *that the most complex series of syllables* (the one with space and color) *was learned more easily than the simplest series; and that both elements in the complication* (as proved in part 1 of space, and in part 2 of color) *contributed to this result.*

II.

Attention as Determined by the Complexity of the Presented Content.

In order to examine the assumption that attention or interest is centered in a complex as opposed to a simple object, several tests were arranged in which a complex and a simple content bid against each other for attention. Experiments relevant to this situation have been made by Mrs. Hill.[1] Her method was this: two cards which first had like figures on them were laid before the subject and he was told to select one by taking it in his hand. Slight differences were then made in the two cards, *e. g.*, a small cross (x) was put in the corner of one card and again the subject made a choice. Her conclusion was that whatever attracted attention was most likely to determine choice in its favor. The general tendency was for attention to go to the card which had had the mark or color added to its other content. Münsterberg[2] has performed experiments to compare the judgment of time-intervals as dependent upon the 'filling' of those intervals. He found that in the case of two equal time-intervals that one was judged shorter which was filled with the more complex content.

The statement is frequently made that, when two fields compete for attention, the intricate or interesting one is preferred. The object of the following tests was to demonstrate as much in the instance of simple geometrical figures. The question as to which of two fields makes the stronger appeal to interest may be divided into two inquiries, namely, which attracts attention first, and which holds attention longer when it is attracted. The distinction is of course a purely practical one. The first point — the attraction of attention — was tested in this way. The subject was asked to fixate a spot marked midway between two simple designs and then to turn the eyes to either side he chose. In order that the first glance might be a real choice he was told that he might look at either picture but at *only one*. The cards were at such a distance apart that a vague impression of the figures could be got in peripheral vision when the

[1] *Am. Jour.*, Vol. 9, p. 587 ff.

[2] Psy. Rev., Vol. 1, pp. 51–56.

eyes were centered on the point between. A few tests were given to each subject in which the two figures were just alike, and in no case did there appear to be any noticeable preference for looking to the right or to the left. Tests were then given in which ten pairs of pictures were shown, each pair consisting of one simple and one somewhat more complicated figure, as in Fig. 3.

The more intricate designs appeared sometimes on the right and sometimes on the left, but not in any regular alternation or

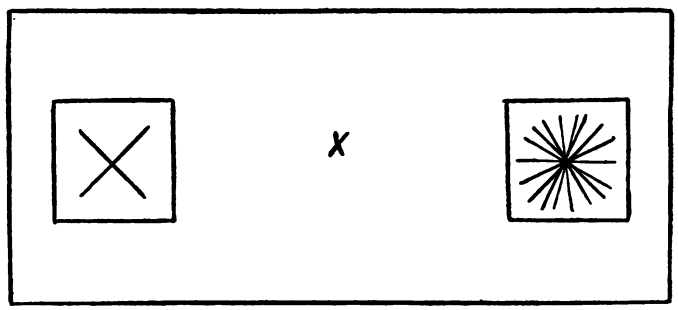

FIG. 3.

rhythm. The question then stood: out of these *ten* possible choices how many times would the subject be attracted by the complex figure?

Out of ten persons, nine chose the complex the great majority of times. Thus, *A* chose the complex nine times out of ten, or 90 per cent. of the time.

A.	*B.*	*C.*	*D.*	*E.*	*F.*	*G.*	*H.*	*I.*	*J.*
90%	100%	60%	90%	90%	90%	10%	100%	100%	100%

The introspective evidence was expressed in terms of 'interest' and 'curiosity.' The subjects were sometimes able during the few seconds of fixation on the mid-point to take in the simpler figure, and the tendency then was to look at the doubtful or indefinite one. The attraction was the indistinctness or the problematic character of the picture. One subject showed a marked exception to the others: *G* almost invariably looked at the simpler figure. His explanation was that the simpler one made a sharper contrast with the background.

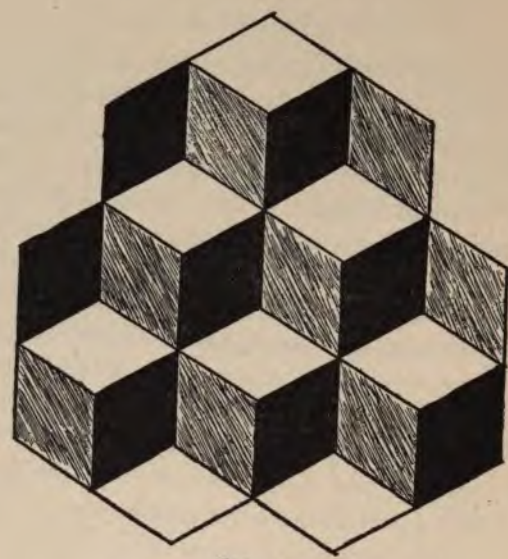

FIG. 4*a*.

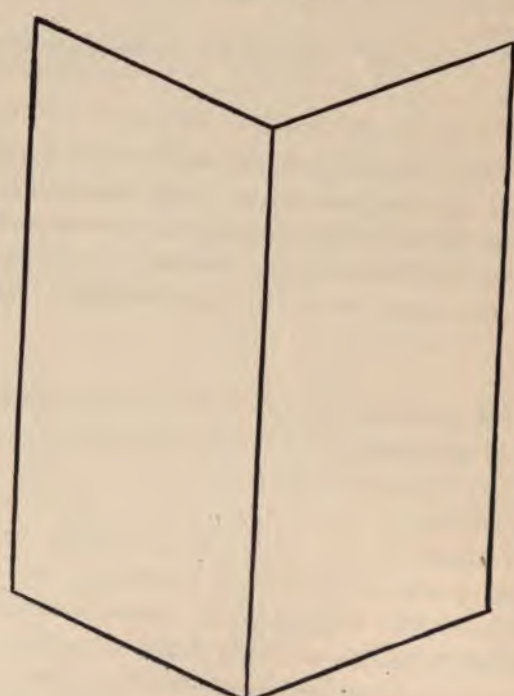

FIG. 4*b*.

the pointer up, and the rate of rotation was kept by a seconds marker so that the results had this form (Fig. 5).

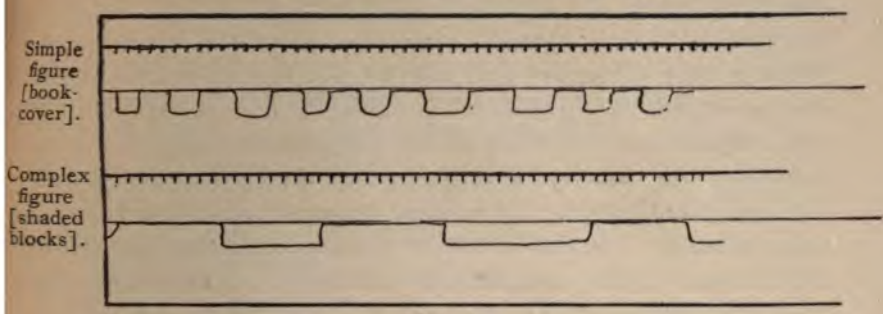

FIG. 5.

The numbers below stand for the number of fluctuations occurring within a given time, *e. g.*, to 100 changes with the simple figure there were only 50 with the complex, which means that the complex one held attention for twice as long as the simpler one did.

	A.	*B.*	*C.*	*D.*	*E.*	
Simple book cover.	100	100	100	100	100	Fluctuations.
Complex shaded blocks.	50	70	35	35	20	"

The actual number of seconds during which the pictures remained constant, was approximately 4, 5 or 6 for the complex, and 2 or 3 for the simple.

To restate the general point of view which the above experiments in memory and attention serve to illustrate, we may consider the relation of meaning as a principle of association to the two principles commonly advanced, namely contiguity and interest. A content has complexity or meaning according to the amount of past experience it represents, or, in proportion to the appeal it makes to habits of mind already fixed. Simple contiguity is the initial type of meaning — its lowest form — a mere chance mechanical juxtaposition: the connection between two things which have an intrinsic interest in each other, or 'necessary' bond, represents the highest or last form of meaning. These two conditions of memory are indispensable to one another. Two contiguous things would never be perceived as

contiguous unless we *attended* to them together, had some slight degree of *interest* in them as together. On the other hand any two things which are intrinsically germane are by that very fact contiguous in consciousness. It seems, therefore, not inappropriate to call contiguity and interest the two extremes of the process by which significance is attained,[1] and to suppose that many repetitions of a thing in our progressive experience may transform the 'merely' contiguous into the 'conatively' or intrinsically contiguous. To a living organism repetition is never mere iteration; but the effects of successive presentations are cumulative. There is added, for example, the emotional element of familiarity. Or, beginning at the other end, we see that discrimination and association go inevitably together (two things held in mind as different are by that act associated), and hence anything which has many differentiæ — much meaning — has many associations, *i. e.*, has been present many times in past experience. The greater its intrinsic appeal, the more must an object have been *worked over* in experience.

On the structural side, then, meaning is contiguity often repeated; on the functional side, it is attention or interest. Repetition is the means to the acquirement of significance, and the fact of internal interest in a content stands for the significance acquired in the past. The two factors are complementary; for with richness of content fewer repetitions are needed to make a lasting impression, whereas a poor material must be repeated many times.

III.

An Investigation of the Process of Judgment as Involved in Estimating Distances.

BY M. L. ASHLEY.

I.

Nature of the Experiment.

It is a well-known psychological fact that our judgments are often due to habits or influences of which we are not at the

[1] Of interest in this connection are a few paragraphs by Stout on the 'acquirement of meaning,' 'Manual of Psy.,' Bk. I., Ch. II.

time fully aware. It is less generally recognized that we may mistake the factor upon which the judgment is really based, and least of all is it commonly supposed that our judgments may rest upon elements which we imagine ourselves to be consciously and definitely neglecting. That such is nevertheless the case is shown by the experiment we are about to describe.

The object of the experiment was twofold. In the first place, it was thought that, if by some means the visual and the tactual-motor factors, upon which we mainly rely in our estimates of depth, could be brought into conflict, the resulting judgments might serve to indicate the respective rôles of these factors in our spatial judgments formed under normal conditions.[1] In the second place, since the judgment would be formed under conflicting tendencies, one might reasonably expect to obtain certain data which would be of value in bringing to light in some degree the psychology of the judging process itself.[2]

In order that these purposes of the investigation might be accomplished, it was essential that the observer should be ignorant of the real situation and should suppose that an object which he reached for and touched was at the same distance from him as an object which he saw. In fact, the problem was to find what effect would be produced on one's judgments by a discrepancy, either recognized or unrecognized, between the visual and tactual-motor distances of objects whose distances were supposed to be the same. Indeed, in most of the experiments the object touched and the object seen were supposed to

[1] Somewhat similar investigations have been made to determine the comparative accuracy of touch and vision, as, *e.g.*, 'A Comparison of Sight and Touch,' by Bowditch and Southard, *Journal of Physiology*, III., pp. 232–245. 'The Perception of Space by Disparate Senses,' Jastrow, *Mind*, XI., p. 539 f. But these experiments were concerned with spatial estimates of the visual and tactual-motor factors when employed separately. Another related experiment is that of Stratton on 'Harmony of Touch and Sight,' *Mind* (N. S.), VIII., p. 492. In this case we have mainly, as in a number of similar investigations, the formation of new habits and coördinations, rather than the direct measuring of one factor against the other.

[2] It may be said of such experiments as K. Marbe's 'Experimentell-psychologische Untersuchungen über das Urtheil,' that they deal with cases of comparatively simple and direct reaction, in which conflicts are not clearly brought out.

be one and the same. To meet these requirements the following apparatus was constructed:

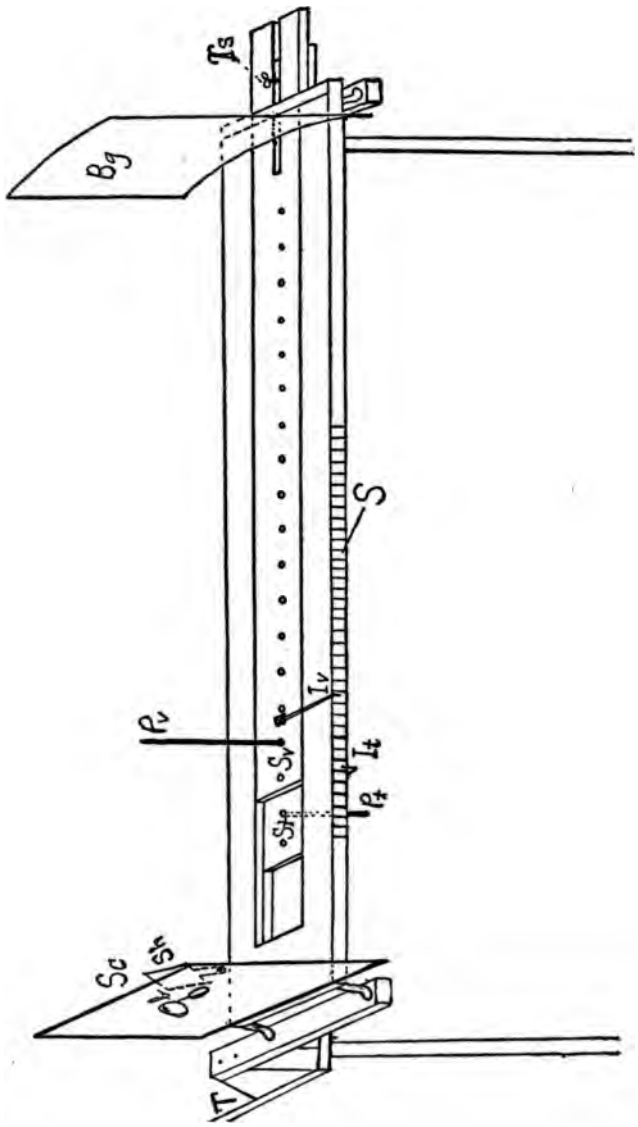

FIG. 1.

Two narrow strips of hard wood were fitted one above the other in a groove in such a manner that they could slide back

and forth either together or independently of each other. Small holes, two centimeters apart, were bored through the two strips, through which a long pin could be pushed, so that it could be seen from above and touched from below. Or, two pins could be used, one of which, *Pv*, would be carried by the upper slide *Sv*, and the other, *Pt*, by the lower slide *St*. *Ts* is a thumb-screw which passes through a slit in *Sv* and clamps *Sv* and *St* together when desirable. The framework with the slides in place is mounted on a table, *T*, in such a manner as to allow freedom for the arm in reaching. At the front is a screen *Sc*, with a narrow opening *Ob*, through which the observer looks with both eyes at *Pv*. *Ob* is provided with a shutter *Sh*. The upper slide is furnished with an index *Iv*, and the lower slide with an index *It*. These indices indicate on the millimeter scale *S* the distance either pin has moved. *Bg* is a black cloth background. The arrangement of the apparatus in the diagram (Fig. 1) shows the lower pin the nearer of the two to the observer.

II.

Experimentation and Results.

In order to determine the degree in which one's spatial judgments could be influenced by the abnormal conditions employed, it was first necessary to find within what limits the movements of the pins could be detected under normal conditions. Since each pin could be moved separately, or both could be moved together, either forward or backward, six difference thresholds would be required for each observer for the distance which was to be employed as a reference point, or standard, from which the pins were to be moved. As a convenient standard distance a point on the scale was chosen and maintained throughout the experiments, which brought the visual pin forty-two centimeters from the eye of the observer.

To obtain the threshold for vision alone the pin *Pv* was exposed a sufficient length of time for the observer to get a clear impression of its location; then the shutter was closed, the pin moved and the shutter opened, about three seconds having been required to change the position of the pin. The observer then judged 'nearer,' 'further,' 'same,' or 'uncertain.' These

judgments are indicated in the tables by *n*, *f*, *s*, and *x* respectively. To find the threshold for the tactual-motor side alone, the shutter was kept closed throughout. After reaching and touching the standard, the observer lowered his arm while the pin *Pt* was being moved. He then raised his arm and after reaching and touching the pin, judged as in the case of vision alone. Practically the same time interval was required for moving the pin as in case of vision. Where vision and touch-movement were combined, the observer saw and touched the pin at the same time. The shutter was then closed and he lowered his arm. The pin having been moved, he again raised his arm and the shutter was opened when he was about to touch the pin, in order that he might receive the tactual-motor and the visual impressions as nearly as possible at the same instant. The time occupied for the shifting of the position of the pin and the form of the judgments were the same as before.

The thresholds were determined mainly by the method of right and wrong cases, the amount of change which was correctly judged in seventy-five per cent. of the cases being taken as the threshold. The unit of change was one half of a centimeter for vision alone or for vision and touch combined; for the tactual-motor form alone the unit employed was one centimeter. During the determination of the thresholds as well as in the subsequent experiments, the observer sat at such a height that his line of vision fell upon the black background and well above the slides which carried the pins. This precaution was taken to prevent his receiving any assistance through comparison with other objects.

When two pins are employed, as in this experiment, there are ten ways in which they can be placed at unequal distances from the observer, who supposes he sees and touches the same pin, while in reality he touches one and sees another. (Only in the exposures employed as standards were the two pins actually at the same distance from him.) These ten positions for the pins are shown below. Suppose the observer to be located at the left of the pins. Where a pin has been moved, its position in the standard will be indicated by a dotted line. Arrows show the direction of movement from the standards which are

figured directly above in each case. These varieties are arranged and numbered in the order in which they were meant to be given, though it was not found convenient to maintain this order strictly throughout the experiment.

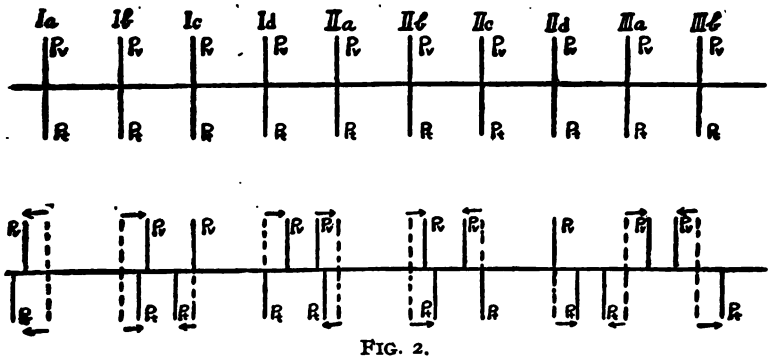

FIG. 2.

It will be noticed that in *Ia*, *Ib*, *Ic* and *Id*, *Pt* is always nearer to the observer than *Pv*, while in *IIa*, *IIb*, *IIc* and *IId*, *Pv* is nearer than *Pt*. In *IIIa* and *IIIb*, *Pv* and *Pt* have moved in opposite directions and their positions in *IIIa* are the reverse of those in *IIIb*.

Since the main object of the experiment was to study the conflicts brought about between the spatial impressions received from *Pv* and *Pt*, the combinations *Ia*, *Ib*, *IIa* and *IIb* were not given so much attention as the others and were omitted after a few trials. Accordingly, it will suffice for our purpose to show in tabulated form the results obtained under *Ic*, *Id*, *IIc*, *IId*, *IIIa* and *IIIb*. These results appear in the following tables, which are numbered in accordance with the previous description. The errors in the judgments will be more apparent if the threshold tests are compared with them, and readily to permit this a table of thresholds will be given first.

THRESHOLDS.

	A.	*B.*	*C.*	*D.*	*E.*
Pvn	1.5	2	1.	1	.5
Pvf	2.5	4.5	1.5	3.5	1.5
Ptn	3	3	4	2	2
Ptf	2	2	3	2	2
P(tv)n	1.5	2	2	1	1
P(tv)f	1.5	3	1.5	2.5	1

Pvn, *Pvf*, *Ptn*, and *Ptf* signify that *Pv* or *Pt* has been moved nearer or farther away. *P*(*tv*) *n* and *P*(*tv*) *f* mean that hand and eye are combined and that the movement is nearer

Ic

Ptn.	*Pvs.*	*A.*	*B.*	*C.*	*D.*	*E.*
1	0	x	n	n	s	s
2	0	x	n	n	n	n
3	0	x	s	n	n	n
4	0	x	n	n	n	s
5	0	x	n	n	n	f
6	0	x	n	n	n	n
7	0	x	n	n	n	n
8	0		n	n	n	n
9	0		n		n	n
10	0		n		n	n
11	0				n	n

Id

Pts.	*Pvf.*	*A.'*	*B.*	*C.*	*D.*	*E.*
0	.5	n	f	n	s	f
0	1	f	f	n	s	s
0	1.5	f	f	s	f	f
0	2	x	f	n	s	f
0	2.5	f	f	f	x	f
0	3	f	f	f	n	f
0	3.5	f	f	f	s	f
0	4	f	s	f	f	t
0	4.5	f	f	f	f	f
0	5	f	f	f	f	f
0	5.5	f	f	f	f	f
0	6	f	s	f	f	f
0	6.5		f	f	f	f
			f		f	t
			f		f	f
					f	f
					f	f

and further respectively. Throughout the tables the vertical columns headed *A*, *B*, *C*, *D*, and *E*, contain the results obtained from the respective observers. In the table for thresholds

IIc.

Pts.	*Pvn.*	*A.*	*B.*	*C.*	*D.*	*E.*
0	.5	x	s	n	n	s
0	1	n	s	n	s	n
0	1.5	n	s	n	s	n
0	2	n	s	n	x	n
0	2.5	n	s	n	n	n
0	3	n	s	n	n	n
0	3.5	n	n	n	n	n
0	4	n	s	n	n	n
0	4.5	n	n	n	n	n
0	5	n	n	n	n	n
0	5.5	n	n	n	n	n
0	6	n	n	n	n	n
0	6.5		n	n	x	n
0	7		f	n	n	n
0	7.5		n		n	n
0	8		n		n	n
0	8.5		n			

IId.

Ptf.	*Pvs.*	*A.*	*B.*	*C.*	*D.*	*E.*
1	0	x	s	n	s	n
2	0	x	s	f	f	f
3	0	x	f	f	s	f
4	0	x	n	f	f	f
5	0	x	f	f	f	f
6	0	x	s	f	f	f
7	0	x	f	f	f	s
8	0	x	f		f	f
9	0	x	f		f	f
10	0	x			f	f
11	0	f				
12	0	x				
13	0	x				
14	0	x				
15	0	x				

the extent of the required movement for each observer is given in centimeters. The letters *Pvn*, *Pvf*, etc., indicate the kind of change for which the threshold was obtained. In the tables

IIIA.

Ptn.	*Pvf.*	*A.*	*B.*	*C.*	*D.*	*E.*
.5	.5	x	s	n	f	n
1	1	x	s	x	f	f
1.5	1.5	f	s	f	x	f
2	2	x	s	f	f	s
2.5	2.5	f	n	f	s	f
3	3	f	s	f	f	f
3.5	3.5	f	n	f	x	n
4	4	f	n	f	f	f
4.5	4.5	f	n		n	f
5	5	f			f	n
5.5	5.5	f			n	f
6	6				n	f
6.5	6.5				s	f
7	7				n	f
7.5	7.5				n	f
8	8					
8.5	8.5					

IIIB.

Ptf.	*Pnv.*	*A.*	*B.*	*C.*	*D.*	*E.*
.5	.5	n	s	s	n	n
1	1	n	s	n	n	n
1.5	1.5	n	s	n	n	n
2	2	n	s	n	s	n
2.5	2.5	n	s	f	n	n
3	3	n	n	f	x	n
3.5	3.5	n	n	f	s	n
4	4	n	f	f	x	n
4.5	4.5	x	s	f	f	s
5	5		s		f	n
5.5	5.5				f	n
6	6				f	n
6.5	6.5				f	n
7	7				f	f
7.5	7.5				f	n
8	8					
8.5	8.5					

Ic, *Id*, *IIc*, *IId*, *IIIa* and *IIIb* the first two vertical columns headed *Pvn*, *Pvf*, etc., contain the extent in centimeters of that particular form of change which the observer judged 'nearer,' 'further,' etc., as the case might be. *Pvs* and *Pts* indicate that *Pv* and *Pt* were stationary.

We see in the table for thresholds that the lowest thresholds were obtained for *Pvn*, while for *Pvf* a much greater change was required.[1] The reverse was true in case of *Ptn* and *Ptf*. The thresholds where eye and hand were used together fall between those in which either eye or hand was used alone. It would appear that in this last case the poorer criterion was not discarded in favor of the better, but entered as a hindrance, and the result was really the combined effect of the two. It should be remarked in this connection that where the change was great, the eye and hand seemed to reinforce each other when the movement for each was in the same direction, and that in such cases there was a marked increase in the observer's assurance.

It was not possible to obtain the same number of judgments of each kind from all of the observers, because the nature of the experiment was such that, if at any time the observer had sus-

[1] This result is in general agreement with those obtained by Wundt, Arrer and others, though the thresholds are rather higher. M. Arrer, 'Ueber die Bedeutung der Convergenz- und Accommodationsbewegungen für die Tiefenwahrnehmung. *Philos. Studien*, XIII., 116–161, 222–304.

pected that the pin he saw was not the same one he touched, his subsequent judgments would have been valueless. Accordingly, when the extent of movement had been sufficiently increased for marked perplexity to be manifest, it became necessary to change to some other form of the experiment. It would hardly be expected that all of the observers could be brought to the same point before recognizing the discrepancy between the two pins.

The tables show that the several observers, despite certain irregularities, were influenced in very different degrees by the visual and the tactual-motor factors. Thus, in table *Ic* (*Pt* nearer and *Pv* stationary) it appears that *A*, who is of the visual type, was in doubt when *Pt* alone had changed its position, while all of the other observers judged more in accordance with the movement of *Pt*. Again, in table *Id* (*Pt* stationary and *Pv* further), *A* judged 'further' quite regularly. In tables *IIc* (*Pt* stationary and *Pv* nearer) and *IId* (*Pt* further and *Pv* stationary), *A's* preference for the visual factor is clearly shown. In tables *IIIa* (*Pt* nearer and *Pv* further) and *IIIb* (*Pt* further and *Pv* nearer), *A* appears to have followed mainly the pin *Pv*. In tables *Ic*, *Id*, *IIc* and *IId*, *B*, *C*, *D* and *E* are shown to have judged for the most part according to the pin which really moved, whether it was *Pv* or *Pt*, though in tables *IIIa* and *IIIb*, *E* seems to have been influenced more by *Pv* than by *Pt*.

An examination of the tables shows further the results of conflicts which were brought about between the visual and the tactual-motor factors. The subjective effects of such conflicts were various. Sometimes the observer seemed to feel no difficulty and to be quite unaware that his grounds of judgment really conflicted. Sometimes he would experience considerable perplexity, but would be at a loss to account for it. In other instances he would assert that he judged by eye or hand almost exclusively, or, again, that he combined the two, while his results indicated clearly that in reality he did not recognize the bases on which his judgments rested.

To make this clearer we may refer to specific instances. In the case of *A's* judgments in table *Ic* (*Ptn* and *Pvs*) the effect of the tactual-motor factor is evident, though throughout the whole experiment *A* supposed her judgments to be almost

wholly controlled by vision. At no time was the difficulty understood, though the perplexity and disappointment at not being able to tell whether the pin had moved or not was clearly shown in the tone of voice in which the judgments were given. It will be seen that in every instance reported in this table *A* was unable to detect any change in the position of the pins, though in some instances the pin *Pt* had actually moved more than twice the distance which could be detected in the threshold determinations in which *Pt* alone or both *Pt* and *Pv* approached (*Ptn* and *P*(*tv*)*n*). The same holds true in a still more marked degree in the experiment reported in table *IId*. Here, though previously able to detect a change of two or three centimeters, an increase of fifteen centimeters in the distance of *Pt* did not lead *A* to judge it further, when *Pv* was stationary. In tables *IIIa* (*Ptn* and *Pvf*) and *IIIb* (*Ptf* and *Pvn*) *A* mainly followed the visual factor, but the results of conflict appear in a few instances, particularly in the last judgment in table *IIIb*.

Though often erratic, *B* judged mainly by the hand. Throughout the tables his judgments are more irregular than *A's*. *B* was inclined to ignore the eye in cases of contradiction and realized, when the discrepancy was considerable, that the visual and the tactual-motor factors did not agree. Tables *IIIa* (*Ptn* and *Pvf*) and *IIIb* (*Ptf* and *Pvn*) give clear cases of conflict between the two elements, in which *B* seemed to follow now one and now the other.

Observer *C* said that his judgments were mainly of an immediate muscular sort wherever the tactual-motor factor was available. He could judge very well by the eye, he thought, but preferred the hand. As his judgments were given with great care, his results and comments are the more significant. Some of his last judgments given in table *Id* (*Pts* and *Pvf*) were ventured with considerable hesitation. Near the end of the series in table *IIc* (*Pts* and *Pvn*) he said that he was laying a good deal of stress on the hand, while, as the table shows, *Pt* was really stationary throughout. Thus, when *Pv* had approached as far as seven centimeters, he seemed to think it was *Pt* which had given him the main evidence of approach. After the last judgment in table *IIIa*, which was given as 'nearer'

(*Ptn* 4.5 cm. and *Pvf* 4.5 cm.), he said he was combining the two elements, and while he thought *vision* the more important factor, he did not disregard the hand. According to his threshold for *Ptn*, he would have confidently judged *Pt* alone to be nearer. At the end of the series of judgments given in table *IIIb* (*Ptf* and *Pvn*), *C* said he judged mainly by touch, and remarked that the tactual object seemed further than the visual one. It appears that at about the middle of the series, where, according to the thresholds, the conflict should have made itself felt, he changed from the visual to the tactual-motor factor as a basis for his judgments, but did not notice the discrepancy between the two pins till near the end. It will be noticed that at the end the pins were actually nine centimeters apart.

Observer *D* placed most reliance on the hand. She remarked once early in the series of table *Ic* (*Ptn* and *Pvs*) that the tactual object seemed nearer than the visual one, but she appeared to disregard this occurrence afterwards. Near the end of table *Id* (*Pts* and *Pvf*), *D* remarked, after having given a judgment 'further' with some hesitation, that the hesitation arose from using hand and eye together, since the visual estimate came more slowly; then, in reply to a question, *D* said that there was *no disagreement* and that *the same result would have been obtained from either alone*. These introspective observations were the more astonishing because *D* had but little faith in the eye compared with the hand as a means of estimating distance. In spite of the fact that *Pt* remained stationary, *D* said the visual estimate of 'further' came more slowly, that there was no disagreement, and that either alone would have given the same result. We have here a clear case of error as to the source from which the judgment was really obtained, and the ascribing to tactual-motor elements of an impression really originating in visual processes.

In table *IIc* (*Pts* and *Pvn*), in the case of the second of the judgments *x*, *D* said that the object as compared with the standard looked the same and felt further. *D* remarked, in addition, that both eye and hand were employed, and that it was not so hard to use both as it had been before. We see, therefore, the same sort of error that was noticed in table *Id* (*Pts* and *Pvf*).

In connection with the results given in *IId* (*Ptf* and *Pvs*), *D* said that in case of any apparent difference at first, the visual object was always made to agree with the tactual, but that the *same final result would have been attained with either alone.* In table *IIIa* (*Ptn* and *Pvf*), *D's* judgments are very irregular, and at the end vision and touch could not be made to agree. The same disagreement was experienced near the end of the series in table *IIIb* (*Ptf* and *Pvn*), though in table *IIIb* the tactual-motor side appears the more influential of the two.

In *E* we have a pronounced visual type. In *Ic* (*Ptn* and *Pvs*), *Id* (*Pts* and *Pvf*), *IIc* (*Pts* and *Pvn*), and *IId* (*Ptf* and *Pvs*) the hand did not appear to play much part and no disagreement was noticed. In table *IIIa* (*Ptn* and *Pvf*), however, this manner of judging seemed to be reversed in a few instances. At the end of another series like *IIIa* and of equal length (not appearing in the tables), in which the first judgment was 'nearer' and all the rest 'further,' *E* said that where the change was great, he tended to judge by means of the hand and had lately been doing so. It is seen, however, that while the pin was being judged further, *Pt* was constantly coming *nearer.* In the last judgment but one in table *IIIb* (*Ptf* and *Pvn*), in which *E* judged 'further,' he noticed the disagreement between the two objects.

In addition to the results which appear in the tables, a few others were obtained from the same observers, which did not differ materially from those tabulated. Other observers also took part in certain phases of the experiment with like general result. In one instance the observer, although he knew the nature of the apparatus, was misled into confidently judging the visual pin nearer, when only the tactual-motor one had approached.

While the experimentation was rather too limited in respect to both the number of observers and the number of judgments obtained to justify any sweeping generalizations, the results are not without significance in regard to certain aspects of our spatial judgments, and are at least suggestive as to the judging process itself. It appears that with persons of either pronounced visual or motor type the corresponding psychical factor tends in judg-

ments of this kind to be emphasized to the neglect of the other in case they are combined. Whether ultimately the visual factor depends on the tactual-motor factor for its real significance, and whether, if cases of marked discrepancy between them were long continued, the visual would yield to the tactual-motor, remains a question. Such a view certainly has much in its favor, and it is not improbable that forms of experimentation could be devised which would go far towards deciding the matter.

The outcome of the experiment certainly adds evidence in support of the view that the bases of our judgments may be in a great measure subconscious, in the sense that they are not explicitly recognized. The introspective testimony given by the observers indicates that, as was mentioned at the beginning, the judgment may be the product of tendencies or habits which we not only do not ordinarily recognize, but which we can not correctly evaluate when conflict brings them to our notice. The most striking of the experiments show that we may be predominantly influenced by factors of which we are clearly conscious and which we definitely suppose ourselves to be disregarding. In fact, the judgment may apparently be viewed as the product of more or less conflicting habits, in which the form it finally takes is a sort of resultant of various forces; and the sharper the conflict among these forces, the greater may be one's sense of uncertainty and distrust as to his correctness.

I am under obligations to those who kindly gave their services as subjects in the experiment. For them the work must have been somewhat uninteresting, inasmuch as they were necessarily kept in ignorance of the real conditions under which they judged and of the real purpose of the investigation. I am indebted to Professor J. R. Angell, not only for various helpful suggestions, but also for considerable assistance in constructing and testing the apparatus.

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF CHICAGO.

COMMUNICATED BY JAMES ROWLAND ANGELL.

EXPERIMENTAL OBSERVATIONS UPON NORMAL MOTOR SUGGESTIBILITY.

BY HAYWOOD J. PEARCE.

The present paper deals with the modifications which certain motor coordinations display under the influence of sensory stimulations of a specific character. The coordinations selected were all movements of localization. The results of the experiments seem to throw an interesting light upon the psychophysical conditions involved in such movements and they indicate the possible nature of some, at least, of the familiar phenomena of suggestion. There appears to be reason to hope that a practicable, even though rough, criterion of general constitutional, or organic, suggestibility may, perhaps, be established along the line of the methods here employed.

The experiments, which grew out of some observations upon the localizing of contact sensations, include, in addition to extensive tests upon the localizing movements of the left hand, when the skin of the right forearm is stimulated, two sets of tests upon eye movements, when these are employed to localize sounds and visual objects. Throughout the paper the emphasis falls purposely upon the qualitative analysis of the conditions involved and not upon the quantitative features of the situation. This statement is not to be interpreted as prejudicing the relia-

bility of such quantitative statistics as are reported. These are all regarded as trustworthy within the limits of the methods adopted. But had the quantitative problems been of fundamental interest, these methods would have been modified in several particulars. The investigation is being carried forward with other and more exact modes of procedure.[1]

The literature bearing most immediately upon the subject of the investigation may be classified under two heads. First, that relating to the effects upon the contractions of both voluntary and involuntary muscles of various psychical conditions and especially the effects of various forms of sensory stimuli. The literature under this head is too extensive to cite exhaustively. Illustrative, however, of the general field are Mosso's observations of the circulatory changes accompanying emotional excitement;[2] Féré's observations upon the shifting power of voluntary muscular contractions under the influence of agreeable and disagreeable stimuli;[3] Mitchell's, Lombard's and Bowditch's studies of the modifications which the knee-jerk manifests under stimulations of both sensory and motor character.[4] Summarizing the positive purport of all these investigations, so far as they bear upon the present experimentation, we may be said to know already that muscular contractions reflect in an extremely sensitive way, capable within limits of quantitative statement, the momentary conditions of the whole nervous system. The cases we shall have to study will be seen to constitute a specific set of instances under this general principle.

As a subordinate investigation under this general head may be mentioned an article by Münsterberg and Campbell upon

[1] I am under obligation to Professor James R. Angell for counsel and assistance throughout the course of the experimentation. My sincere thanks are also due the persons who so kindly served as subjects for me, to wit: Misses M. Graves, E. Bartlett, E. Pearce, J. Biggars; Messrs. J. R. Angell, E. H. Mahood, C. A. Woolsohn, W. C. Gore, J. H. Lathrop, L. D. Arnett, E. L. Kimball, R. Clark, M. Hall, R. C. Adams, W. A. McKeevor; Masters H. Pearce and T. Pearce.

[2] Mosso, Kreislauf d. Blutes im Mensch. Gehirn.

[3] Féré, Sensation et Mouvement.

[4] Mitchell, *Medical News* (Philadelphia), 1886 (Feb. 13 and 20).
Lombard, *Amer. Jour. Psy.*, I., p. 1.
Bowditch, *Boston Medical and Surgical Journal*, 1888 (May 31).

the motor power of ideas.[1] I have adopted in part of my experiments an apparatus similar in many particulars to that used by these writers and by a different method have confirmed what they sought to establish. By varying the method I have also secured new results, which afford considerable assistance in interpreting the Münsterberg-Campbell observations.

The second main group of papers in the relevant literature concerns the discriminative sensibility of the skin. In the tactual experiments I have adopted methods somewhat similar to those used by E. H. Weber and later by Volkmann, Henri, Judd and others.[2] Further than a general similarity of method our experiments have little, if anything, in common. Such occasional points as are significant for my investigation will be more conveniently referred to in connection with the report of the experiments.

The rapidly enlarging field of literature on the subject of suggestion has necessarily had a general influence in determining my attitude with reference to the interpretation of my observations. As most prominent, perhaps, in this respect may be mentioned Sidis' 'Psychology of Suggestion,' Schmidkunz's 'Psychologie der Suggestion' and Binet's 'La suggestibilité.'

Dermal-Motor Suggestibility.

The dermal experiments number over four thousand and were made upon six persons, four men and two women. The conditions were varied slightly from time to time, as will be pointed out later, but in general they were as follows. The subject, being seated, was required to bare his right arm to the elbow and place the same upon a low table or other support, palm upward, and in as comfortable a position as possible. The stimulations were distributed equally over the region of the mid-arm, the wrist and that near the elbow, being given in the order named. The subject's eyes being closed, a fairly vigorous pressure was made upon the arm with a blunt instrument

[1] Münsterberg and Campbell, Psych. Rev., I., p. 441. Cf. also the drastic criticism of this paper by Nichols, *Phil. Rev.*, IV., p. 174.

[2] Henri, L'Année Psychol., II., p. 295. Also, Die Raumwahrnehmungen des Tastsinnes. This monograph contains an excellent bibliography of the subject. Judd, *Philos. Studien*, XII., p. 409.

and the subject was required immediately to touch as near the same point as possible with a similar instrument held in his left hand. He was allowed only one trial, *i. e.*, the point where he first touched was the one noted, and the error both in amount and direction was recorded. The normal displacement, *i. e.*, the average error when a single stimulus was localized, was first determined. To determine this ten tests were made with a single stimulus in each of the three regions of the arm named—the stimulations being given first on mid-arm, then on wrist, then near elbow, the process being repeated ten times. Then the same process was repeated with the addition of a second stimulus 6.5 cm. below the first, given immediately following, or in the case of some subjects simultaneously with the one to be localized.[1] Thus a series of ninety tests was completed. The experiments at each sitting usually occupied one hour, during which time from ninety to one hundred tests were made.

Various simple devices were employed for giving the stimulations. In the majority of the experiments an ordinary pair of æsthesiometric compasses was used, the points of which are spherical and about a millimeter in diameter. The points are likely to set up slight temperature sensations in addition to the touch sensation and in some of the series of tests wooden points were substituted. For the purpose of the investigation this is really a matter of no great consequence.

In making up the tables, it will be observed, I have disregarded errors made to right and left of the point to be localized. These would only complicate the situation. When the localization was exactly right or left the error is regarded as zero; if the localization was down or up *and* to right or left, the error down or up only is considered. These errors to right and left have apparently no uniformity and I could not see that they were materially altered when the second stimulus was given. The amount of such lateral errors varied from zero to about twelve millimeters, but was generally nearer the former figure.[2]

[1] This distance is considerably in excess of that where the two points are felt as one by most observers.

[2] The practical justification of this procedure is supported by Lewy's investigations—*Zeitsch. für Psychol. u. Physiol. d. Sinnesorg.*, VIII., p. 231. He found these errors which I disregard as few in number and insignificant.

Only two of the subjects were aware of the purpose of the experiments. They were instructed to attempt as much as possible to disregard the second stimulus, *i. e.*, not to allow it, consciously, to influence them in either direction. In other words, they were required to make the reaction in as reflex a manmer as possible. The subjects who did not know the object of the experiments were told that the purpose of the second stimulus was to test the effect of distraction, to see if they could localize as well with as without a second stimulus. When questioned after all of the experiments had been completed, these subjects were still totally ignorant of the real purpose as well as the *result* of the experiments. All the subjects agreed that, so far as they could judge, the only effect of the second stimulus had been to render a trifle more difficult the identification of the point which they sought to touch. In order to test the accuracy of this testimony I had one of the subjects make a series of the tests on myself and I was unable to tell, until after I had examined the figures, what the nature of my own reactions had been. One subject had considerable difficulty in distinguishing the two stimuli as two when they were about six centimeters apart and given near the elbow. It was interesting to observe, however, that even under such conditions his reactions were practically as accurate as when the two points were clearly distinguished. This serves to illustrate the essentially reflex nature of these reactions.

In the experiments which have been recorded in the literature of this subject, the emphasis has, so far as I am aware, been placed upon the sensory side—upon the accuracy or inaccuracy of the sensory discrimination of the points stimulated. In my experiments this feature has been of secondary, even of incidental interest. I have worked upon the theory that every sensation and every image of a sensory stimulus has a more or less definite motor element of a kind tending to release movements of localization. In reflex action, for example, a motor impulse is somehow more or less definitely coordinated with a particular sensory stimulus. Disregarding the question of how the particular coordinations under discussion arise, it has been my purpose to determine the accuracy of

this motor impulse, and later, to determine how such impulses may be modified by other similar stimuli both successive and simultaneous. The subject, therefore, was not allowed to move the pointer after it had once touched the arm. This was frequently quite unsatisfactory to the subject and some subjects were anxious for further trial, believing they could do better. In many instances this was undoubtedly true—the subjects had a fairly definite idea of the direction and the amount of the error. But in many other instances they were quite satisfied with the result and in still other cases, when questioned or given permission to try again, their judgment was quite at fault. This last was particularly true if the first error was very small. On the whole, I think the tabulated results will show that the average errors in localization made by using this method are not very much greater than the errors recorded by others, who have allowed the subject to keep trying until satisfied; but it is not claimed that the figures represent in any degree the discriminative sensibility of the skin as a sense-organ. To be sure Henri comments on the great uncertainty attaching to our judgments of such localizing movements.[1] It is not entirely clear from his description how closely his conditions are comparable with mine. Apparently the localizations he refers to were made by pointing and not by effecting actual contact. This would certainly introduce an important difference. In any case the facts are as just stated. The difference in the accuracy of the localizations under the two modes of procedure seemed neither marked nor constant, and as the point to be investigated was the effect of the secondary stimulus upon the movement, rather than the sensitivity of the skin, the method adopted seemed entirely justifiable.

Results.

The first table presented (Table A.) shows the averages obtained from each subject. It will be noticed that with a single stimulus all of the subjects except one displace downward, *i. e.*, toward the wrist. V. Henri,[2] in his experiments, to which refer-

[1] Henri, Raumwahrnehmungen d. Tastsinnes, p. 114.
[2] 'L'Année Psychologique,' Vol. II., p. 177.

TABLE A.—TACTUAL.[1]

Region of Arm.	Single Stimulus.				Suggestion Up.							Suggestion Down.						
	Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive Influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg Influence.	Positive Influence in cm.	Neg. Influence in cm.
Forearm.	60	.78	D	.57	70	.20	U	.77	74	26	.80	.02	70	1.08	D	.61	61	39	.46	.26
"	114	.72	D	.54	119	.77	U	.79	82	18	1.46		118	1.20	D	.82	68	32	.63	.23
"	57	.54	U	.42	58	1.06	U	.44	79	21	.98		58	1.86	D	.43	98	2	1.93	
"	96	.84	D	.71	111	.31	U	.72	84	16	1.11	.08	98	1.05	D	.64	64	36	.61	.31
"	131	.73	D	.51	156	1.51	U	.61	93	7	2.02	.01	163	1.02	D	.59	56	44	.79	.53
"	150	.39	D	.64	153	1.37	U	.80	90	10	1.77		159	.44	D	.58	44	56	.38	.38
"	236	.07	D	.66	221	1.54	U	.75	89	11	1.62		219	.57	D	.61	67	33	.69	.26

ence has already been made, found that his subjects usually displaced in the direction of a 'point of reference,' which they had in mind in making the localization. This point of reference was usually a bone or some prominent configuration of the surface of the skin. If, however, this point of reference was a considerable distance from the point touched, the error was usually made in the opposite direction. If the subjects use a point of reference, the fact noted by M. Henri appears to be quite reasonable, and I think the cause is to be found in the fact that the image of the point of reference itself has a localizing motor element and this merged with the corresponding element from the sensation or image of the point stimulated is quite sufficient to account for the error in that direction.[2] The use of reflective judgment, however, in my experiments was eliminated, and my subjects apparently did not have in mind any point of reference of the kind which M. Henri describes. How then shall we account for this almost uniform tendency to displace in the direction of the hand, even when the stimulus was given near the elbow? It is quite possible, of course, that the hand is always used, even unconsciously, as a point of reference when the localization is made on the forearm, but I think there is reason to believe that another factor plays a more conspicuous part. It is well known

[1] The figures showing the average error, mean variation, etc., are given in centimeters.

[2] The analogy between such a 'point of reference' and the zero point or focus of attention at the beginning of the auditory and visual experiments is manifest. See pp. 343 and 350.

that the nerve terminations in the skin are more numerou
we pass downward toward the extremities. Further, it has o
been noted that in stimulating the skin we do not stimu
merely a point, but, on account of the tension of the skin,
stimulate an area. Inasmuch, then, as the nerve supply in s
an area is greater in the lower half, it is quite conceivable
the attention should be focused not on its mathematical cer
where the point was stimulated, but on a center of equilibri
which must, by reason of the nerve distribution, fall in
lower half. President Hall, in explaining similar facts brou
out in some of his investigations,[1] finds their origin in
genetic development of the race. Erroneous estimations of
sensations of movement involved in the localizations have
been adduced as responsible for the tendencies manifested.

Referring again to Table A, we find that when the sec
stimulus is given above the first, the error downward is char
into an error upward, and that in the single case where the
ural error was up, the error is considerably increased when
second stimulus is given. On the other hand, when the sec
stimulus is given below the first, the natural error downwar
considerably increased in the direction of the second stimu
Table A contains a summary of all of the experiments.
first series of experiments does not show the same results as
later ones. In the first experiments the disposition to resist
influence of the second stimulus, and even to make an erro
the opposite direction, was quite manifest. These results b
included in the averages shown in Table A, have the effe
decreasing the average value of the influence of the sec
stimulus. It was my first intention, therefore, to leave ou
consideration the results of the first series, regarding ther
mere control experiments, but the gradual yielding to the
gestion as it is repeated time after time appears to me no
one of the most significant facts developed during the cours
the experiments. In order, therefore, to show the change in
form of reaction, I present Table B, which shows all of
series of reactions obtained from a single subject. It wil

[1] See Hall and Donaldson, *Mind*, Vol. X., p. 557; also Hall and Mc
Amer. Jour. of Psychology, Vol. I., p. 72.

observed that when the second stimulus[1] is given above the first, it begins to have a consistent positive influence after the second series, and that when the second stimulus is given below the first, a consistent positive influence is not manifest until the middle of the fourth series. This table is fairly typical of the results derived from each subject. The subject is a graduate student of philosophy, accustomed to critical introspective analysis. He was not cognizant of the purpose of the experiments, and when questioned at the end was, like all the others, ignorant of the effect produced by the application of the second stimulus.

TABLE B.—TACTUAL.—SUBJECT G.[2]

Series.	Region of Arm.	Single Stimulus.				Suggestion Up.							Suggestion Down.						
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation.	Per cent. of Pos. Influence.	Per cent. of Neg. Influence.	Positive Influence in cm.	Neg. Influence in cm.
I	Mid-arm.	9	2.13	D	.92	9	.52	D	.80	89	11	1.61		9	.54	U	.44		100		2.67
	Wrist.	7	.20	U	.47	7	1.81	U	.41	100		1.61		7	2.20	U	.78		100		2.00
	Elbow.	9	1.82	D	.55	9	.06	D	.86	89	11	1.76		9	2.95	D	.65	89	11	1.13	
II	Mid-arm.	9	.33	U	.67	9	.03	U	.67	45	55		.30	9	.29	U	.33	45	55		.06
	Wrist.	8	.50	U	.20	8	2.60	U	.72	100		2.10		8	2.15	U	.67		100		1.65
	Elbow.	9	1.60	D	.87	8	.37	D	.37	75	25	1.23		8	3.61	D	.58	100		2.01	
III	Mid-arm.	10	.60	U	.30	10	3.01	U	.69	100		2.41		10	1.32	D	.66	100		1.92	
	Wrist.	9	1.03	D	.47	9	2.53	U	.90	100		1.50		16	.59	U	.62	7	93		1.62
	Elbow.	8	1.17	D	.52	9	3.43	U	.88	100		2.26		9	2.60	D	.42	100		1.43	
IV	Mid-arm.	7	1.17	D	.57	7	1.55	U	.71	100		2.72		9	1.10	D	.95	55	45		.07
	Wrist.	9	.88	D	.27	9	2.55	U	.77	100		3.43		8	.62	U	.63		100		1.50
	Elbow.	10	2.01	D	.67	10	1.54	U	.60	100		3.55		9	2.98	D	.72	90	10	.97	
V	Mid-arm.	8	.55	D	.37	9	1.98	U	.27	100		2.53		9	1.47	D	.53	78	22	.92	
	Wrist.	15	.56	U	.90	7	1.53	U	.60	86	14	.97		9	.31	U	.63	55	45	.05	
	Elbow.	8	.21	D	.46	9	1.44	U	.42	100		1.65		8	2.86	D	.43	100		2.65	
VI	Mid-arm.	8	1.15	D	.25	8	1.62	U	.52	100		2.77		8	2.03	D	.51	87	13	.88	
	Wrist.	10	.49	D	.39	10	2.00	U	.70	100		2.49		9	.63	D	.41	55	45	.14	
	Elbow.	9	1.22	D	.35	9	.55	U	.41	90		1.77		9	3.38	D	.70	100		2.16	

Variation in the intensity of the second stimulus affects the influence of the stimulus. Three different intensities were tried, —less, equal, and more intense than the first stimulus. I offer only a single illustration. When the second stimulus was less

[1] In the case of this subject, the two stimuli were given simultaneously throughout the experiment. Occasional observations have been made showing a tendency, when two such stimuli are felt as one point, to locate this midway between the two points actually touched. Henri's experiments do not confirm these reports. *Cf.* Henri, Raumwahrnehmungen d. Tastsinnes, p. 130.

[2] The average errors, mean variation, etc., are given in centimeters.

intense than the first, it produced a change in the reaction as compared with the normal of 1.51 cm.; when it was equal in intensity to the first, its effect was 2.17 cm.; and when more intense than the first, the effect was 3.00 cm. A change in the distance of the second stimulus from the first also affects the result. The greatest effect is produced when the second is distant from the first about 8 cm. in the case in which the second stimulus is given above the first. In the other case the greatest effect is produced at a distance of about 10 cm. Either increasing or diminishing this distance diminishes the influence of the second stimulus. The reason for the decrease in effect when the distance between the two points decreases is apparently simple and may be best explained according to the principle of the parallelogram of forces. But the other case is not so clear. I take it, however, that when the distance is greater than 10 cm., the sensory quality of the two stimuli becomes so very different, that attention is more definitely aroused and the compensatory tendencies, which were so manifest in the earlier experiments, are again more definitely engaged in the reaction. These tendencies are very manifest when one stimulus is given on the arm and the other on the palm of the hand—the sensory quality of the two regions being distinctly different. It appears quite probable, however, that even in these cases, if the experiments were continued long enough for the two qualities of sensation to become habitually associated, so that attention is no longer disturbed by the difference in quality, these compensatory tendencies would gradually disappear and we might thus finally say that the effect of the second stimulus is proportional to its distance from the first. The foregoing statement certainly agrees with, and possibly in a measure explains, the change in the direction of the error in the early stages of the experiments, to which reference has already been made.

Later Tactual Experiments.

For the purpose of comparing the results of the tactual with the auditory and visual experiments which are to follow, I have carried on another series of tactual experiments, in which I have endeavored to use a method similar to that which

I found it expedient to adopt in the case of the auditory and visual investigations. The subjects were two boys, aged respectively eight and seven years. They also served as subjects for the auditory and visual experiments, and hence the results from all three lines of experiments are comparable.

The method used was as follows. Beginning at a distance of 6.5 cm. from the junction of wrist and palm of hand, on the volar surface of the forearm, I marked off five lines parallel to each other, one centimeter apart, and perpendicular to the long axis of the arm. The stimulus to be localized was given on one of these lines, beginning with the one nearest the wrist and proceeding upward in regular order. Sufficient extra stimuli were introduced in irregular order to prevent the subject from forming too definite a conclusion as to the order. This precaution was observed in all the experiments. The same process as that just detailed was again repeated with the addition of a second stimulus given immediately after and 6 cm. below the first. The stimulus acted as a suggestion 'down.' The process was again repeated, but this time the second stimulus was given 6 cm. above the first and acted as a suggestion 'up.'

The accompanying table (C) represents three complete series for both subjects of ten tests each for each of the five standard points.

TABLE C.—TACTUAL.[1]

Subject.	Single Stimulus.				Suggestion Down.						Suggestion Up.						Per cent. of Suggestibility.	
	Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M Variation.	Positive Influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence in cm.	Neg. Influence in cm.		
T. P.	50	.4	D	.8	50	.8	D	.8	.4		50	.7	U	.9	1.1		68	
	50	.8	D	.9	50	1.0	D	.6	.2		50	.4	U	.6	1.2		84	
	50	.7	D	.6	50	1.4	D	.6	.7		50	1.8	U	.8	2.5		82	78
H. P.	50	.9	U	.8	50	2.0	U	.6		1.1	50	2.4	U	2.0	1.5		51	
	50	.7	U	.7	50	1.2	U	.9		.5	50	2.6	U	.8	1.9		61	
	50	.6	U	.8	50	.2	U	.9	.4		50	1.6	U	.8	1.0		76	63

[1] The average errors, mean variations, etc., are given in centimeters.

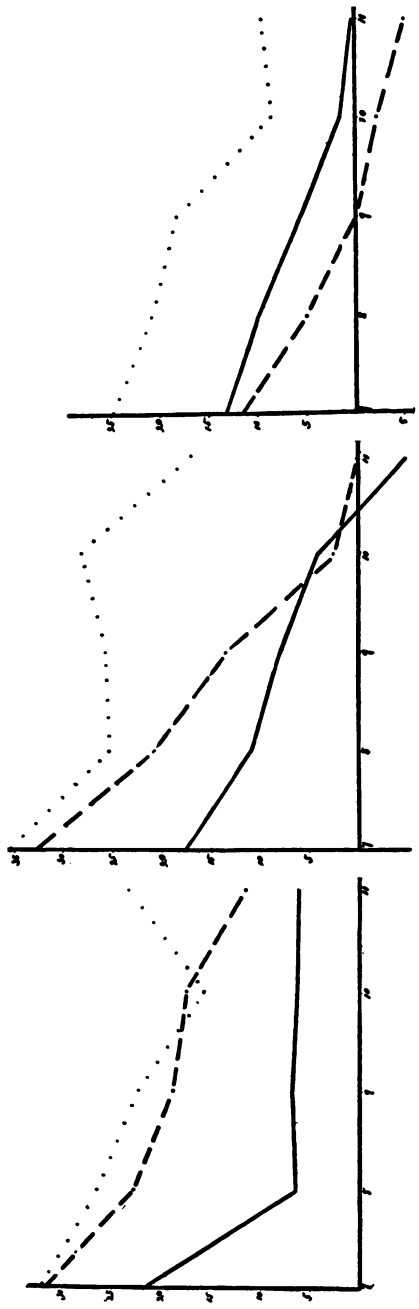
It will be noticed that the second subject, H. P., contradicts what I have previously said as to the direction of the normal error downward.[1] Again, in the first series, when suggestion 'down' is given, *i. e.*, a suggestion opposed to his normal tendency, he reacts against the suggestion in the opposite direction. This contradiction is decreased in the second series and the suggestion is fully effective in the third series. On the other hand, it is interesting to observe that the suggestion in the direction of his normal tendency, *i. e.*, up, decreases in force from the first to the third series. I shall have occasion to note and comment upon similar variations in the other experiments. The fact that such variations are constant under such diverse conditions would appear to have considerable significance.

In order to portray graphically the change in the character of the reactions, as well as the other features of the experiments, I have constructed curves representing each of the three series for the subject H. P. The points on the ordinate, viz. 7, 8, 9, 10, 11, represent the points on the forearm where the stimuli were given. These were respectively 7, 8, 9, 10 and 11 cm. distant from the juncture of wrist and palm. The figures on the abscissa represent the error in localization measured in millimeters. The general downward tendency of all of these curves is particularly remarkable, inasmuch as it shows that the absolute error in localization decreases as we proceed upward toward the elbow. This is probably due to a variation in the force of some motor element, which is not clearly apparent. There was no reason to suspect any peculiarity in the sensory discrimination for the region and this motor factor is seemingly the only direction in which an explanation could be found.

Summary of Results.

I. In the localization of single points stimulated on the forearm there is a general tendency to displace downward, *i. e.*, in the direction of the hand.

[1] I could discover nothing in the conditions of the experiment which would account for this contradiction. Outside of the possibility of the subject's using a different 'point of reference,' which I think improbable, I know of no way to account for the contradiction, except by reference to the general character of the subject.



MEAN VARIATION.

Subject H. P. (Tactual) Fig. 1. Fig. 2. Fig. 3.

——— Single Stimulus, 7.7 7.2 8.0

– – – – Suggestion Down, 6.4 9.8 9.1

. . . . . Suggestion Up, 10.5 8.2 8.6

Average Suggestibility, 51% 76%

II. If a second stimulus is given *above* the first and the subject is required to localize the first stimulus, the normal error downward at the outset is *increased;* if the second stimulus is given *below* the first and the subject is required to localize the latter the normal error downward is at the outset *diminished.*

III. But, after a time, varying with each subject, the tendency just mentioned is reversed. A second stimulus below the first increases the normal tendency to displace downward and a second stimulus above the first changes the tendency to displace downward into a tendency to displace upward, *i. e.*, in the direction of the second stimulus. This second tendency is apparently permanent, so long as the stimulations are repeated. Whether reversion to the original tendency would ever occur cannot be said.

IV. The absolute effect of the second stimulus when given below is less than when given above the point to be localized. In other words, a suggestion to increase the normal tendency to error is less effective than a suggestion to reverse the nature of the error.[1]

V. The distraction of attention produced by the second stimulus is shown by a slight increase in the average variation. This increase is greater when the suggestion is 'up,' *i. e.*, in the direction contrary to natural displacements.

VI. The effect of the second stimulus is proportional to its intensity relative to the first.

VII. The greatest effect of the second stimulus results when it is given at a distance of eight to ten centimeters from the first.

Auditory.

The second series of experiments represents an effort to determine the influence of a sound, when the attempt is made to locate another immediately preceding sound of like character. The subjects used were eleven in number, seven men, two women and two boys. The two boys and three of the men also served for the visual experiments, thus affording some basis for comparison.

[1] This principle will be slightly modified in connection with the auditory and visual tests.

The apparatus employed in the experiments was constructed as follows: A semicircular table, six and a half feet in diameter and three and one-third feet high, bore on its outer circumference and extending above it to a height of six inches, sheets of white cardboard, on which were drawn the degrees of a circle. Back of this cardboard was a black curtain extending entirely around the outer circumference of the table and elevated two feet above it, thus concealing entirely the movements of the operator from the subject, as well as affording a homogeneous background. The subject is seated in the center of the circle, of which the table is a part, so that his eyes are equidistant from all points on the graduated cardboard, behind which the sounds are to be made. The sound, a short, sharp noise, is made with a metallic clicker commonly used in practicing the Morse telegraphic code. The eyes of the subject, at a given signal, are fixed upon a cross placed just above the zero point[1] in front of him and in the center of the scale. After two seconds a click is given at some point behind the graduated cardboard in the right hand quadrant. The subject immediately follows the impulse to turn his eyes in the direction of the sound heard and calls the number below the point upon which his eyes rest. In the first experiments I selected ten definite points at which the sounds were always given. Later, I found it more expedient to reduce the number to five. After a single stimulus was given at each of these points, the same series was repeated, but another click was made at the subject's left, usually thirty degrees from the first click.

With the first group of subjects used, I unfortunately adopted the plan of giving the distracting stimulus first, and the stimulus to be localized last. This was done for the purpose of reducing the variable error to the minimum. Under these conditions, suggestion to the right, after the first few tests, is always effective, but in the majority of cases the suggestion to the left, *i. e.*, toward the center, produces contradictory results—the localization is more accurate than with a single stimulus. The reason is plain. The attention is first carried to the distracting stimulus. But this point is nearer the point to be localized than

[1] See note on p. 335.

the zero point from which the attention should start. Hence the motor power of the distracting stimulus is *subtracted* from the motor power of the zero point, whereas, if the distracting stimulus is given after the stimulus to be localized, its motor power is *added* to the motor power of the zero point. With the second group of subjects, therefore, I adopted the latter method of procedure. The five points, which were to localized, were as follows: 30, 40, 50, 59 and 69 degrees. The sounds were usually given at these points in the order named, only such variation and introduction of extra stimuli being employed as were thought necessary to prevent the subject from being sure of the relative direction and distance of the succeeding sounds. None of the subjects were aware of the fact that constant points were selected, nor were they acquainted with the nature of the influence which the second stimulus was expected to exert. On being questioned at the close of the last series of experiments, the eleven subjects were somewhat divided in their opinion as to the effect of the second stimulus. One was aware of a tendency to follow the second sound; others thought it would probably have an opposite effect. But the majority thought that its only effect was to make the reaction somewhat less accurate. These impressions evidently resemble those reported in the tactual experiments.

Results.

Turning to the results themselves, I present Table D. This represents all of the tests with the second group of subjects, except a few made to determine the effect of variation as regards the distance of the stimuli apart, their relative intensity, etc. Reading through, for example, the first series of tests obtained from H., a man, we find that in 50 tests, when a single stimulus was given at the five points already named, he displaced the sound on the average 4.6 degrees to the left, *i. e.*, toward the center or zero point from which his eyes started. The mean variation for this series was 2.7 degrees. Again, in 50 tests, when a second stimulus was given 30 degrees to the left of the first, he displaced the first stimulus on the average 8.8 degrees to the left, showing that the second or suggesting stimulus has had a positive effect of 4.2 degrees on the average for each time

TABLE D.—AUDITORY.[1]

Series.	Subject.	Single Stimulus.				Suggestion to Left.					Suggestion to Right.					Per cent. of Suggestibility.	
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence.		
I	H	50	4.6	L	2.7	50	8.8	L	3.6	4.2	50	1.3	L	3.5	3.3	69	
II	H	50	3.4	L	3.2	50	8.5	L	3.3	5.1	50	2.8	L	4.9	.6	66	
III	H	50	14.1	L	2.9	50	16.0	L	3.1	1.9	50	.8	L	4.5	13.3	84	73
I	A	50	12.5	L	4.5	50	15.6	L	3.1	3.1	50	11.7	L	3.8	.8	70	
II	A	50	16.6	L	2.9	50	17.1	L	3.4	.5	50	6.9	L	4.2	9.7	72	
III	A	50	14.1	L	2.9	50	16.0	L	2.1	1.9	50	.8	L	4.5	13.3	84	75
I	M	50	5.2	L	6.6	50	8.3	L	3.0	3.1	50	3.3	L	4.5	1.9	69	
II	M	50	5.8	L	3.4	50	9.0	L	3.5	3.2	50	1.3	L	5.3	4.5	73	
III	M	50	6.2	L	3.9	50	10.1	L	3.9	3.9	50	2.0	L	3.0	3.2	86	76
I	T. P.	50	2.0	L	4.9	50	8.8	L	7.4	6.8	50	2.1	L	5.9	0	64	
II	T. P.	50	3.3	L	5.3	50	7.2	L	4.3	3.9	50	.3	R	6.8	3.6	72	
III	T. P.	50	2.6	L	4.9	50	5.5	L	5.9	2.9	50	2.8	R	6.9	5.4	62	66
I	H. P.	50	5.1	L	4.3	50	6.3	L	5.0	1.2	50	.9	L	6.1	4.2	63	
II	H. P.	50	3.0	L	6.5	50	6.8	L	6.2	3.8	50	1.1	L	6.4	1.9	64	
III	H. P.	50	4.3	L	3.1	50	7.8	L	5.1	3.5	50	2.2	R	7.3	6.5	72	66

it was given; mean variation 3.6 degrees. Similarly, in 50 tests, when the suggestion was to the right, there was a displacement of only 1.3 degrees to the left, showing a positive effect of 3.3 degrees for each suggestion, the mean variation with suggestion to right being 3.5 degrees. From this series I have calculated that H. showed a suggestibility of 69 per cent. This calculation was made in the following way: The normal error for a single stimulus at a particular point was determined by averaging the individual errors made at that point. This normal error was then compared with *each* error made at the same point under the influence of suggestion. If this suggestion is to the left and the error is now greater than the normal, it is considered that the suggestion has operated; if the suggestion is to the right and the error is now less than the normal, or if the usual error to the left has become an error to the right, *i. e.*, in the direction of the second stimulus, the suggestion is again consid-

[1] The figures showing average error, mean variation, etc., represent the degrees of a circle.

ered to have operated. Thus, comparing the number of times the suggestion was given with the number of times it proved effective, we secure an index of the suggestibility of the individual, at least for this particular kind of suggestion.

Turning now to the table (D) as a whole, we find that without exception all of the subjects, when a single stimulus is given, displace it toward the left, *i. e.*, in the direction of the center upon which the subject's eyes have been fixed. For the sake of comparison, I made a few experiments in the left quadrant and in this case the error is uniformly to the right—again toward the center. Binet, expressing a familiar view, has said that every image has a motor element, and Münsterberg and Campbell have made experiments, to which reference has already been made, showing, as they maintain, the 'motor power of ideas.' The fact just noted appears to sustain the theory of both of these contentions. It certainly appears insufficient to say that the inertia of the ocular apparatus is wholly responsible for this falling short in the localization of sound under the conditions described, though it is quite possible that this factor, together with the increased muscular effort necessary to turn the eyes toward the periphery, is partly responsible for the nature and the size of the error. It would appear, however, if Münsterberg and Campbell are right in attributing a certain motor power to the idea of the object from which the eye starts in its movement, that this motor power ought to be, under the same conditions, a constant quantity. At first glance the more detailed results of my experiments appear to contradict this theory. But I believe that this apparent contradiction is really a further confirmation. The detailed results show that the size of the error to the left increases with the distance from the center. This increase in the size of the error is not due to increase in the motor power of the original object of attention. But besides the increased muscular exertion made necessary by increased distance,[1] which might partially account for the change, there is also to be considered the increase in the number of ob-

[1] In a few exceptional cases the error decreased after the stimulus passed 45 degrees. This is possibly explained by the fact that the head was turned in that direction and the proportion of muscular effort necessary to turn the eyes was thus decreased.

jects intervening between the zero point and the point from which the sound arises. Each of these points, when imaged upon the retina, has its own motor power. This fact was well illustrated in a difficulty which I encountered with my apparatus as first constructed. The numbers were written by "fives" upon the graduated cardboard, and the line representing each fifth degree was slightly elevated above the intervening ones. I found that the subject almost invariably localized the sound at one of these prominent points and consequently I altered the apparatus, so that all the lines representing the degrees were equally prominent and were extended twelve inches upon the table and there numbered. Thus the subject was required first to select the line and then to trace it to the end, before he could determine the number which it represented.

Another interesting fact is revealed by examination of Table D. There is an evident tendency in the three series of tests for the positive influence of the suggestion, when given to the left, to decrease as the experiment proceeded from Series I. to III.; whereas, on the other hand, there is an equally evident tendency towards an increase in positive influence when the suggestion is made toward the right. When it is remembered that 'toward the left' is the normal tendency to error and that in connection with the tactual experiments I said that a suggestion opposed to a normal tendency was more effective than the reverse, the real significance of the fact just pointed out becomes more apparent. We shall later find the same fact manifest in the visual experiments.

There is usually a slight increase in the mean variation when an interrupting suggestion is given, and as a rule the increase is greater when the suggestion is toward the right, *i. e.*, opposed to the normal tendency. All of the subjects except one show an increase in suggestibility from Series I. to III. The tendency to vacillation is sometimes very great at the first sitting and this tendency sometimes continued throughout an entire series of experiments, which usually occupied two or three days. The exception noted above is due doubtless to the conditions under which the third series of experiments was made. They were more hurried than usual and outside disturbances were more annoying.

In order to represent graphically the facts, which are brought out less clearly by the tabular statement just presented, I present some curves showing the progress of the experiments with a single subject, these curves being fairly typical of the results obtained from each of the subjects. In Figs. 4–6 are shown three curves. The points at which the stimuli were given are represented on the ordinate as 30, 40, 50, 59, 69, these figures representing degrees of a circle, counting from the center of the table, where the subject's eyes are fixed upon the zero point, to the right. The degrees of error in localization are represented on the abscissa as 5, 10, 15, 20, etc. The unbroken line represents the curve of normal error when a single stimulus is given; the broken line represents the curve of error when the suggestion is made toward the left; and the dotted line represents the error when the suggestion is made toward the right.

It will be noted that in the three series the normal error remains relatively about the same, the distance between the normal curve and the curve of suggestion to right increases. The general downward tendency of all of the curves is manifest.

Visual.

The problem here is to determine the effect of a second visual stimulus upon the localization of an object seen in peripheral vision. The object to be localized as well as the object used for suggestion was a piece of white paper 1 cm. square, pasted on a black cardboard 2 cm. square. These squares were attached to wooden rods, twelve inches long, which were arranged to slide noiselessly in grooves provided for the purpose, upon the outer circumference of the same table described in connection with the auditory experiments. The operator stood behind the screen and by pushing up one of the rods caused the visual stimulus to appear just above the edge of the semicircle. The stimulus appeared against the black cloth screen, which shielded the experimenter from the view of the subject. The points selected for giving the stimulus were 30, 40, 50, 59 and 69 degrees, the same as in the auditory experiments. After some preliminary experiments, 15 degrees was

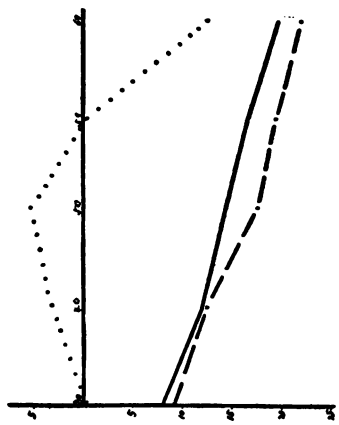
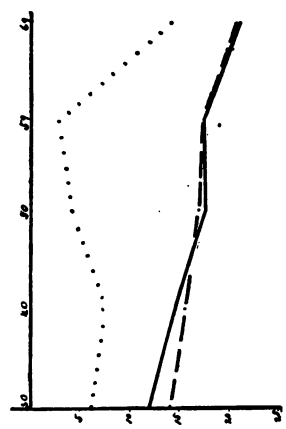
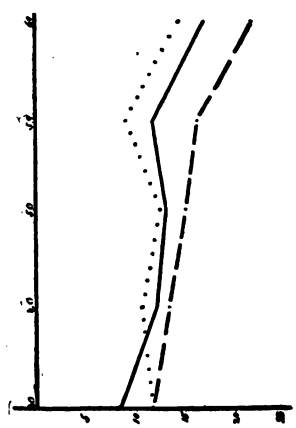


Fig. 6.
2.9 deg.
3.1 "
4.5 "
84%



MEAN VARIATION.

Fig. 5.
2.9 deg.
3.4 "
4.2 "
72%



Subject A (Auditory). Fig. 4.
——— Single Stimulus, 4.5 deg.
– – – – Suggestion to left, 3.1 "
. . . . . Suggestion to right, 3.8 "
Average Suggestibility, 70%

selected as the most convenient distance to maintain between the two stimuli. The stimulus to be located appeared first and the suggesting stimulus immediately thereafter. The conditions observed in the auditory experiments were maintained as nearly as possible in the visual tests. A square similar to that to be located was placed permanently above the cardboard at zero in the center of the table. At a given signal the subject fixated the square.[1] Two seconds thereafter a stimulus appeared at 30 degrees, continuing in the field of vision not more than two seconds. Immediately after its disappearance the subject followed the impulse to turn his eyes in the direction where the object appeared. He noted the line nearest the point where the object seemed to be, followed the line down upon the table and reported the number corresponding to it. After stimuli had thus appeared at each of the five points already named, the process was repeated in the same order with the addition of suggestions to the left, *i. e.*, immediately after the first stimulus was given at 30 degrees, a second stimulus appeared at 15 degrees, etc. The subject had been previously instructed not to move his eyes, until after the disappearance of the second stimulus. After suggestions to the left had been given in connection with each of the five fixed points, the same process was again repeated, save that the suggestions were now made to the right. Except with one subject the start was always made at 30 degrees and the series progressed regularly to 69 degrees outward. In the case of the exception just noted, suggestion to the right, under these conditions, had the effect contradictory to that usually produced in the case of the other subjects. But if, after reaching 69 degrees when giving suggestion to the left, I reversed the order and gave suggestion to the right, working from 69 degrees to 30 degrees, the results compared very favorably with those derived from the other subjects. I can attribute this result only to individual peculiarity. If translated into terms of ordinary life, it would seem to indicate that the subject is almost abnormally opposed to innovations, that it is with great difficulty he is influenced to alter his habits, etc. I questioned him concerning these matters at

[1] See note on p. 335.

the close of the experiments, and his account of his feeling with reference to such things agrees exactly with such a presupposition. It was interesting to observe, however, that even in the case of this subject, before the end of the experiments, the influence of the suggestion to the right, *i. e.*, opposed to his normal tendency, became much greater than the influence of suggestion to the left, even showing some abnormality here again. Thus we see that though a suggestion opposed to the normal tendency may be less effective in the beginning than a suggestion which agrees with a normal tendency, ultimately the principle which I have already enunciated obtains, even in such abnormal cases.

RESULTS.

To show the results of all the visual experiments at a glance, I present Table E. The subjects are the same as those used in the auditory experiments, the conditions were the same as nearly

TABLE E.—VISUAL.[1]

Series.	Subject.	Single Stimulus.				Suggestion to Left.						Suggestion to Right.						Per cent of Suggestibility.	
		Tests.	Av. Error.	Direction.	M. Variation.	Tests.	Av. Error.	Direction.	M. Variation.	Positive Influence in cm.	Neg. Influence in cm.	Tests.	Av. Error.	Direction.	M. Variation	Positive Influence in cm.	Neg. Influence in cm.		
I	H.	50	.6	L	1.3	50	3.0	L	1.5	2.4		50	0		1.6	.6		61	
II		50	.4	L	1.4	50	1.6	L	1.1	1.2		50	.8	R	1.3	1.2		72	
III		50	.5	R	.8	50	.2	L	.7	.7		50	.3	R	1.2		.2	54	62
I	A.	50	4.0	L	2.2	50	5.6	L	1.5	1.6		50	6.5	L	1.7		2.5	47	
II		50	5.0	L	1.3	50	6.5	L	1.2	1.5		50	5.3	L	2.4		.3	63	
III		50	4.5	L	1.1	50	5.2	L	1.0	.7		50	1.7	L	2.4	2.8		71	
IV		50	3.8	L	.9	50	1.6	L	1.1	.8		50	1.6	L	1.8	2.2		70	63
I	M.	50	5.2	L	1.3	50	7.4	L	1.4	2.2		50	5.0	L	1.5	.2		66	
II		50	5.1	L	2.0	50	6.1	L	1.7	1.0		50	3.8	L	2.3	1.3		69	
III		50	4.5	L	1.6	50	5.1	L	2.0	.6		50	2.6	L	2.6	3.9		65	67
I	T. P.	100	2.5	L	2.0	100	5.8	L	2.9	3.3		100	.9	L	2.8	1.5		80	
II		50	2.5	L	1.9	50	4.7	L	1.9	2.2		50	1.2	R	2.6	3.7		79	
III		50	.7	L	1.9	50	3.6	L	2.3	2.9		50	.9	R	2.3	1.6		76	78
I	H. P.	100	6.0	L	1.7	100	6.6	L	2.6	.6		100	4.6	L	2.0	1.4		54	
II		50	2.9	L	1.5	50	3.2	L	1.7	.3		50	1.7	L	1.4	1.2		51	
III		50	2.3	L	1.5	50	.9	L	3.3		1.4	50	.4	L	2.5	1.9		49	55

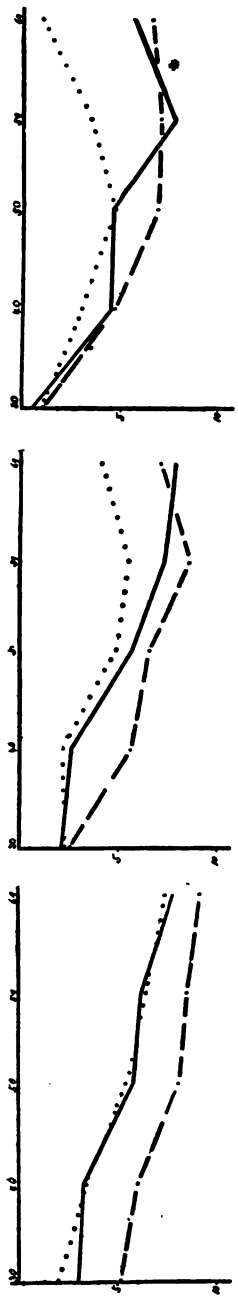
[1] Figures showing error, mean variation, etc., represent degrees of a circle.

as possible, and the results are of a remarkably similar character. As might be expected, the absolute error is less in the case of the visual tests, as is also the mean variation. The percentage of suggestibility is less in the case of the visual than in the case of the auditory series. This may be accounted for partly by the fact that the second stimulus in the visual experiments was only 15 degrees distant from the first, while in the case of the auditory experiments the distance between stimuli was 30 degrees. Taking into consideration the fact, which was established, viz., that the effect of the second stimulus varies with its distance from the first and also with its relative intensity, the difference in the percentage of suggestibility is not at all remarkable. It will be remembered that the absolute distances selected for the two groups of experiments were determined with reference to avoiding large variations in the localizations.

Showing the results of the visual experiments in a more graphic manner, I present some curves constructed similarly to those already shown for the auditory experiments. Figure 7 represents the curve of suggestibility of subject M., a man, as indicated by the first ten tests for each of the five standard points. Figures 8 and 9 represent curves of suggestibility for the same subject as indicated by the second and third series of ten tests each, for each of the five standard points. In comparing the auditory and visual curves it will be noticed that the scale on which they are constructed has been altered, the errors being somewhat exaggerated in the case of the visual.

Referring to the curves we see again the increase in error toward the periphery, shown by the gradual slope downward of all the curves; again we notice a gradual diminution in the power of suggestion, when given in the direction of the normal tendency; while the corresponding increase in power, when the suggestion is opposed to the normal tendency, is equally manifest.

The first effect of any change in the method of the experiments, *e.g.*, a change in the distance between the first and second stimulus, is to produce contradictory results. Attention is aroused and the subject reacts against the suggestion. I found it difficult, therefore, to get trustworthy results showing the effect of the change in distance between the two stimuli. To



MEAN VARIATION.

Subject M (Visual). Fig. 7.

—— Single stimulus, 1.3

– – – – Suggestion to left, 1.4

. . . . . Suggestion to right, 1.5

Average suggestibility, 66%

Fig. 8.

2.0

1.7

2.3

69%

Fig. 9.

1.6

2.0

2.6

65%

get perfectly reliable results it would be necessary to perform the same number of experiments for each of the distances selected, and, perhaps, to have experiments at all the distances selected at each sitting. This would have hindered the chief purpose of my experiments and would have involved more time than I had at my disposal. However, from a limited number of experiments, I think I am safe in saying that a change in the distance between the stimuli results in a corresponding change in the amount of error. For example, with one subject, I found that in fifty tests, with the two stimuli 20 degrees apart, the total effect of the second stimulus amounted to 16.8 degrees; while, in the same number of tests, with a distance of 15 degrees between stimuli, the total effect of the second stimulus amounted to 14.9 degrees. There would obviously be upper and lower limits for the operation of such a principle.

Summary of all Results.

1. In localizing a single stimulus, whether tactual, auditory or visual, an error is ordinarily made in the direction of the point upon which attention is directed when the stimulus is given. This error notably increases with the distance of the stimulus from the point of attention.

2. When a second stimulus of like character to the first is given, there is at first manifest a tendency to resist the suggestion thereby offered. But this tendency is diminished as the suggestion is repeated, and ultimately the suggestion becomes fully effective, causing the subject to err in the direction of the second stimulus.

3. The resistance, or the contradictory tendency, is most vigorous when the suggestion is made in a direction opposed to the normal tendency to error;[1] but ultimately a suggestion opposed to a normal tendency is more effective than in the reverse case.

4. Variation of intensity and distance of second stimulus, within certain limits, is followed by a corresponding variation in effect produced. The first effect, however, of any change in the method is to produce vacillating and contradictory results.

[1] This is true certainly in the auditory and visual experiments; less certainly in the case of the tactual.

It would evidently be difficult to hit upon a wholly satisfactory basis for comparing the suggestibility manifested by the three tracts of the nervous system investigated in the present experiments. Adopting the rough working principle of percentages employed in this paper, it appears that there is no exact correspondence among the several forms of tests in the case of any of the persons who served as subjects. Nevertheless the correspondence is close enough to be decidedly significant of considerable homogeneity in this respect throughout the nervous system. More notable, perhaps, is the fact that the same person shows the highest degree of suggestibility in each form of the experiment. Furthermore, if we compare the percentages of any two individuals in the different forms of the test, we shall find the ratios remarkably similar. The single exception to this statement occurs in the case of one of the subjects (T. P.) whose hearing was defective and whose results in the auditory series cannot fairly be used. For example, the percentages of one subject were for the visual tests 55 and for the auditory 66. For another subject the figures are 62 and 73. The proportion thus becomes 62:73::55:66. This gives us 4,092 = 4,015. The proportions are not all as nearly exact as this one, but the principle involved seems to hold throughout. So far, therefore, as the present observations permit a conclusion, they serve to indicate that conflicting stimuli provocative of localizing movements possess relatively equal values, whatever portions of the human nervous system they may effect. Given the degree of suggestibility of two individuals for one of the three tracts of the nervous system, we may reasonably infer relatively equal degrees of suggestibility for the other two tracts. This, in any event, is the obvious implication of my results, subject always to such modification as specific circumstances may involve.

Such a criterion of natural suggestibility seems to possess considerable advantage in point of exactness over that used by Binet in his tests upon school children.[1] He investigated the amount of modification he could introduce by suggestive questions into the judgments the children made of the lengths of visual lines. Such a method, while giving interesting indica-

[1] Binet, 'La suggestibilité,' p. 20.

tions of the general differences among children of different ages and education, clearly involves many shifting conditions of a purely social kind, which jeopardizes seriously the definiteness of the results. My method is not applicable immediately to large groups. But the results of it are certainly free from some important sources of error incidental to Binet's method. It is in this connection not without interest, perhaps, that, in the case of the two young boys who served for me, my results confirm very strikingly the opinion of all who know them well, as regards their relative sensitiveness to suggestion. In the present status of the subject we must frankly admit that any criterion is wholly tentative. I venture, however, to call attention to the fact that the parallelism is more than hypothetical between the conditions of my experiments and the conditions of an individual suddenly immersed in a new social and moral environment. Both are submitted to stimulations of whose exact purport they are often quite unaware. The first reaction is one of withdrawal and aversion. But under repeated exposure to the stimulus, without any explicit consciousness of what is occurring, they gradually yield and take on a new habit, which is the expression of a process that has hardly at any point involved reflective consciousness.[1]

If the theories of hypnotic suggestion, which have been couched in terms of inhibition, can give a really satisfactory account of the suppression of inhibitive associative processes, such facts as are reported in this paper would seem to show that the nervous system is entirely capable of executing the remaining activities, which have to be accounted for in explaining the effectiveness of such suggestion. This matter has been sufficiently discussed, as regards the psychophysical mechanism concerned, in the body of the paper and need not be again subjected to comment. The motor impulses intrinsic, in adult life at least, to sensations and images contain the clue to adequate explanation.

[1] In connection with reflective consciousness, attention may be directed to the process of the judgment involved in such experiments as these. In cases of this kind there is essentially a balancing of motor tendencies within the nervous organism and the resultant of these tendencies *is* the judgment. As a concrete instance of one form of relationship between judgments and motor activities it is not altogether devoid of interest.

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